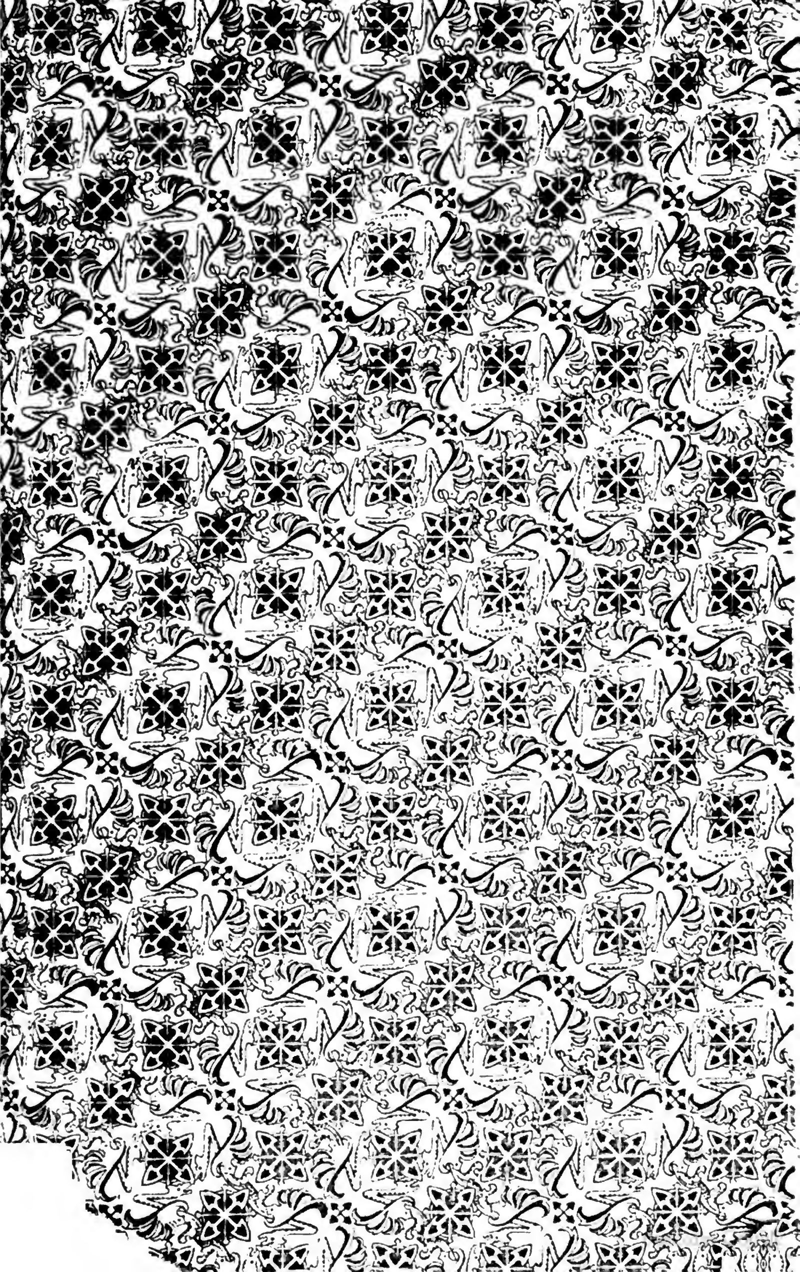

PROCEEDINGS

OF

THE AMERICAN ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE.

FOURTH MEETING,

HELD AT NEW HAVEN, CONN., AUGUST, 1850.

WASHINGTON CITY.
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1851.

EDITED BY

SPENCER F. BAIRD,

Permanent Secretary.

EDWARD O. JENKINS, PRINTER,
114 Nassau street, N. Y.

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AT THE

NEW HAVEN MEETING, AUGUST, 1850.

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Prof. JAMES HALL, of Albany.
Prof. BENJAMIN SILLIMAN, Jr., of Albany.
Prof. WALTER R. JOHNSON, of Washington, D. C.

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Dr. Thomas Hun.
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Prof. John Foster.
Prof. Jonathan Pearson.
Prof. Green.
Dr. Joel Wing.

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Committee to prepare Resolutions relative to the Continuance of the Magnetic and Meteorological Observatory at Toronto.

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Prof. Joseph Henry.

Committee on the Establishment of a Zoological Journal. (Reported and discharged.)

Prof. J. D. Dana. | Dr. Jos. Leidy.
Prof. S. F. Baird. | Prof. L. Agassiz.
Prof. Jeffreys Wyman.

Committee on the Paper of Prof. Mitchel, upon a new Method of Observing and Recording Right Ascension and North Polar Distances.

Prof. B. Peirce. | Prof. J. H. Alexander.
Prof. W. H. C. Bartlett. | Dr. H. J. Anderson.
Prof. Samuel St. John.

Committee on Prof. Coakley's Elliptic Tables of Neptune.

Prof. B. Peirce. | Lieut. Chas. H. Davis.
Prof. A. D. Bache.

Committee to Memorialize Congress on the subject of attaching a Corps of Scientific Observers to the Mexican Boundary Survey.

Prof. L. Agassiz. | Prof. W. B. Rogers.
Prof. B. Silliman, Sen. | Dr. Wolcott Gibbs.
Dr. S. G. Morton.

Committee to examine the Experiments of Prof. Johnson on Coals, and to express an opinion as to the propriety of their continuance by order of Congress.

Dr. Morrel Wyman, Boston. | R. C. Taylor, Esq., Philadelphia.
Prof. B. Silliman, Jr., New Haven. | Prof. J. P. Norton, New Haven.

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Prof. Benjamin Peirce, Cambridge.
Prof. Caswell, Providence.
Prof. Wm. B. Rogers, Va.
Prof. M. J. Williams, S. C.
Prof. Chas. F. McCay, Athens, Ga.
Israel W. Andrews, Esq., Ohio.

Committee to Memorialize the Legislature of Pennsylvania in reference to the publication of the final Geological Report of that State.

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Solomon W. Roberts, Esq., Philadelphia.
Prof. A. D. Bache, Washington.
Pres. Ed. Hitchcock, Amherst.
Wm. C. Redfield, Esq., New York.
Prof. Jas. Hall, Albany.

Sub-Committee to make Arrangements for Reporting Proceedings of the Albany Meeting.

Prof. D. Olmsted, of Yale.
Prof. S. F. Baird, *Permanent Secretary.*

Sub-Committee, in the name of the Standing Cammittee, to revise, alter, adopt, and publish the Rules of Organization, presented at the meeting of the Standing Committee, held on Monday, August 26.

Prof. A. D. Bache.
Prof. S. F. Baird.
Prof. Jos. Henry.

Sub-Committee to report a List of Colleges whose Professors in branches of Science are Members of the Association, with the Names of the Professors, and also the Names of those who are not yet Members.

S. F. Baird, *Permanent Secretary.*

Sub-Committee to prepare and present to the Association a System of Scientific Ethics.

Prof. Joseph Henry.

COMMITTEES CONTINUED FROM THE NEW HAVEN MEETING.

On the subject of a Uniform Standard of Weights and Measures.

On the Coast Survey.

On Memorializing Congress in relation to Scientific Explorations.

On a Change in the Constitution providing for Honorary Members.

On Physical Constants.

The following amendments to the Constitution were proposed, to be acted on at the Albany meeting, August, 1851:—

1st. That the Permanent Secretary shall erase from the list of members the names of all those who, by the return of the Treasurer, shall appear to be two years in arrears for annual dues;—suitable notice being given by two circulars from the Treasurer, at an interval of three months, to all who may fall within the intent of this provision.

2d. That the annual subscription of each member of the Association be two dollars, which shall entitle him to a copy of the proceedings of each annual meeting.

OBJECTS AND RULES OF THE ASSOCIATION.

OBJECTS.

The Society shall be called "The American Association for the Advancement of Science." The objects of the Association are, by periodical and migratory meetings, to promote intercourse between those who are cultivating science in different parts of the United States; to give a stronger and more general impulse, and a more systematic direction to scientific research in our country; and to procure for the labors of scientific men increased facilities and a wider useful-

RULES.

MEMBERS.

Rule 1. Those persons, whose names have been already enrolled in the published proceedings of the Association, and all those who have been invited to attend the meetings, shall be considered members, on subscribing to these rules.

Rule 2. Members of scientific societies, or learned bodies, having in view any of the objects of this Society, and publishing transactions, shall likewise be considered members, on subscribing to these rules.

Rule 3. Collegiate Professors of Natural History, Physics, Chemistry, Mathematics, and Political Economy, and of the theoretical and applied Sciences generally; also, Civil Engineers and Architects who have been employed in the construction or superintendence of public works, may become members, on subscribing to these rules.

Rule 4. Persons not embraced in the above provisions, may become members of the Association, upon nomination by the Standing Committee, and by a majority of the members present.

OFFICERS.

Rule 5. The officers of the Association shall be a President, Secretary, and a Treasurer, who shall be elected at each Annual Meeting, for the meeting of the ensuing year.

MEETINGS.

Rule 6. The Association shall meet annually, for one week or

longer, the time and place of each meeting being determined by a vote of the Association at the previous meeting; and the arrangements for it shall be entrusted to the officers and the Local Committee.

STANDING COMMITTEE.

RULE 7. There shall be a Standing Committee, to consist of the President, Secretary, and Treasurer of the Association, the Officers of the preceding year, the Chairmen and Secretaries of the Sections, after these shall have been organized, and six other members present, who shall have attended any of the previous meetings; to be elected by ballot.

RULE 8. The Committee, whose duty it shall be to manage the general business of the Association, shall sit during the meeting, and at any time in the interval between it and the next meeting, as the interests of the Association may require. It shall also be the duty of this Committee to nominate the General Officers of the Association for the following year, and persons for admission to membership.

SECTIONS.

RULE 9. The Standing Committee shall organize the Society into Sections, permitting the number and scope of these Sections to vary, in conformity to the wishes and the scientific business of the Association.

RULE 10. It shall be the duty of the Standing Committee, if, at any time, two or more Sections, induced by a deficiency of scientific communications, or by other reasons, request to be united into one; or if at any time a single Section, overloaded with business, asks to be subdivided, to effect the change, and, generally, to readjust the subdivisions of the Association, whenever, upon due representation, it promises to expedite the proceedings, and advance the purposes of the meeting.

SECTIONAL COMMITTEES AND OFFICERS.

RULE 11. Each Section shall appoint its own Chairman and Secretary of the Meeting, and it shall likewise have a Standing Committee, of such size as the Section may prefer. The Secretaries of the Sections may appoint assistants, whenever, in the discharge of their duties, it becomes expedient.

RULE 12. It shall be the duty of the Standing Committee of each Section, assisted by the Chairman, to arrange and direct the proceedings in their Section, to ascertain what written and oral communications are offered, and, for the better forwarding the business, to assign the

order in which these communications shall appear, and the amount of time which each shall occupy; and it shall be the duty of the Chairman to enforce these decisions of the Committee.

These Sectional Committees shall likewise recommend subjects for systematic investigation, by members willing to undertake the researches, and present their results at the next Annual Meeting.

The Committees shall likewise recommend Reports on particular topics and departments of science, to be drawn up as occasion permits, by competent persons, and presented at subsequent Annual Meetings.

REPORTS OF PROCEEDINGS.

RULE 13. Whenever practicable, the proceedings shall be reported by professional reporters or stenographers, whose reports are to be revised by the Secretaries before they appear in print.

PAPERS AND COMMUNICATIONS.

RULE 14. The author of any paper or communication shall be at liberty to retain his right of property therein, provided he declares such to be his wish before presenting it to the Society.

GENERAL AND EVENING MEETINGS.

RULE 15. At least three evenings in the week shall be reserved for General Meetings of the Association, and the Standing Committee shall appoint these and any other General Meetings which the objects and interests of the Association may call for.

These General Meetings may, when convened for that purpose, give their attention to any topics of science which would otherwise come before the Sections; and thus all the Sections may, for a longer or shorter time, reunite themselves to hear and consider any communications, or transact any business.

It shall be a part of the business of these General Meetings, to receive the Address of the President of the last Annual Meeting, to hear such reports on scientific subjects as, from their general importance and interest, the Standing Committee shall select; also, to receive from the Chairmen of the Sections, abstracts of the proceedings of their respective Sections, and to listen to communications and lectures explanatory of new and important discoveries and researches in science, and new inventions and processes in the arts.

ORDER OF PROCEEDINGS IN ORGANIZING A MEETING.

RULE 16. The Association shall be organized by the President of the preceding Annual Meeting: the question of the most eligible distribution of the Society into Sections shall then occupy the attention of the Association, when, a sufficient expression of opinion being procured, the meeting may adjourn, and the Standing Committee shall immediately proceed to divide the Association into Sections, and to allot to the Sections their general places of meeting. The Sections may then organize by electing their officers, and proceed to transact scientific and other business.

LOOAL COMMITTEE.

RULE 17. The Standing Committee shall appoint a Local Committee from among members residing at or near the place of meeting for the ensuing year; and it shall be the duty of the Looal Committee, assisted by the officers, to make arrangements for the meeting.

SUBSCRIPTIONS.

RULE 18. The amount of the Annual Subscriptions of each member of the Association shall be one dollar, which shall entitle him to a copy of the proceedings of each meeting. The Members attending an Annual Meeting shall pay, on registering their names, an additional assessment of ——— dollars. These subscriptions to be received by the Treasurer or Secretary.

ACCOUNTS.

RULE 19. The accounts of the Association shall be audited annually, by Auditors appointed at each meeting.

ALTERATIONS OF THE CONSTITUTION.

RULE 20. No article of this Constitution shall be altered or amended, without the concurrence of three-fourths of the members present, nor unless notice of the proposed amendment or alteration shall have been given at the preceding Annual Meeting.

MEMBERS

OF THE

AMERICAN ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE.

NOTE.—Names of deceased members are marked with an asterisk, (*) and those of members who, in 1840, formed the original "Association of American Geologists," are in small capitals.

A.

Abbott, Dr. S. L., Boston.
Abert, Col. J. J., Washington, D. C.
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Adams, Penteg'st., Esq., Jeff'n co., N. Y.
Adams, John G., Esq., New York.
Agassiz, Prof. Louis, Cambridge, Mass.
Aiken, Hon. Wm., Charleston, S. C.
Aikin, Prof. W. E. A., Baltimore.
Albro, Rev. Dr. J. A., Cambridge, Mass.
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Alexander, John H., Esq., Baltimore.
Alexander, Dr. R. C., Bath, England.
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Allen, J. L., Esq., New York.
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Bachman, Dr. John, Charleston, S. C.
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Bacon, Pres. ——? Washington, D. C.
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Barker, Dr. Sanford, Charleston, S. C.
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Barratt, Dr. Joseph, Middletown, Ct.
Barber, Rev. Isaac R., Worcester, Mass.
Barbour, J. R., Esq., Worcester, Mass.

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Beck, Dr. T. Romeyn, Albany, N. Y.
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Beck, Prof. Lewis C., N. Brunswick, N. J.
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Bellinger, Dr. John, Charleston, S. C.
Benedict, Prof. Farr'nd, N. Burl'gton, Vt.
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*Binney, Dr. Amos, Boston,
Binney, John, Esq., Boston, Mass.
Bigelow, Dr. Jacob, Boston.
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Blake, Wm. P., Esq., New York.
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Blake, John L., Esq., Boston.
Blake, Eli W., Esq., New Haven, Ct.
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Budd, Dr. B. W., New York.
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Cabot, J. Elliott, Esq., Boston.
Cabot, Samuel, Jun., Esq., Boston, Mass.
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Campbell, Dr. A. B., Philadelphia.
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Cappy, Dr. J. P., Montgomery, Al.
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Clay, Joseph A., Esq., Philadelphia.
Clarke, Prof. W. W., Albany.
Clarke, Alvan, Esq., Cambridge.
Clarke, Prof. F. N., West Point, N. Y.
Clark, Wm. S., Esq., East Hampton.
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Cleaveland, Dr. A. B., Dedham, Mass.
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Coates, Dr. Reynell, Philadelphia.
Coan, Rev. Titus, Hilo, Hawaii.
Coffin, Prof. James H., Easton, Pa.
Coffin, Prof. J. H. C., Washington, D. C.
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Cole, Thomas, Esq., Salem, Mass.
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Cooper, Wm., Esq., New York.
Cooke, George H., Esq., Troy, N. Y.
Cooke, Josiah P., Esq., Cambridge, Mass.
Cotting, Dr. B. E.,——? Georgia.
Couthouy, Joseph P., Esq., New York.
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Craw, Wm. J., Esq., New Haven.
Cresson, J. C., Esq., Philadelphia.
Crossley, Richard, Esq., Boston.
Culmann, R., Bavaria.
Curtis, Rev. M. A., Society Hill, S. C.
Curtis, Dr. Josiah, Lowell, Mass.
Curtis, L. W., Unionville, S. C.

D.

Dale, L. D., Esq., New York.
Dana, Rev. J. J., South Adams, Mass.
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Winchell, Alex., Esq., Eutaw, Ala.
Wing, Matthew, Esq., Albany.
Wing, Dr. Joel, Albany.
Winslow, Rev. G., Staten Island, N. Y.
Wolf, Dr. Elias, New York.
Woodhull, Lieut. Maxwell, U. S. N.
Woolsey, Prof. Theodore D., New Haven.
Worcester, Dr. Jos. E., Cambridge, Mass.
Woodbury, Hon. L., Portsmouth, N. H.
Wragg, Dr. W., Charleston, S. C.
*Wright, Dr. John, Troy, N. Y.
Wright, A. D., Esq., Brooklyn, N. Y.
Wurtz, Jacob H., Esq., New York.
Wyman, Dr. Jeffries, Cambridge.
Wyman, Dr. Morrill, Cambridge.

Y.

Yates, Giles F., Esq., Albany, N. Y.
Yeadon, Richard, Esq., Charleston, S. C.
Young, Prof. Ira, Hanover, N. H.

Z.

Zimmermann, C., Esq., Columbia, S. C.

LIST OF MEMBERS ELECTED AT THE NEW HAVEN MEETING.

Anderson, Dr. Henry T.
Bagg, Dr. Moses M., Utica, N. Y.
Barnard, Henry, New Haven.
Boynton, John F., New York.
Bradley, Francis, Esq., New Haven.
Brewer, Wm. H., New Haven.
Brush, George J., New Haven.
Bunce, John B., New Haven.
Burnet Hon. Jacob, Cincinnati.
Cappy, Dr. T. P., Montgomery, Ala.
Chnoweth, B. D., Esq., Winchester, Va.
Clarke, Prof. W. W., Albany.
Clarke, Alvan, Esq., Cambridge.
Clarke, Prof. F. N., West Point, N. Y.
Cogswell, Dr. Mason F., Albany.
Craw, Wm. F., Esq., New Havan.
Culmann, R., Bavaria.
Day, Dr. Jeremiah, New Haven.
Dayton, A. O., Esq., Washington City.
Dwight, Timothy, Esq., New Haven.
Ely, Chas. Arthur, Esq., Elyria, Ohio.
Erni, Dr. Henri, New Haven.
Este, D. K., Esq., New Haven.
Evans, Hon. Alex. C., Esq., Newark, N. J.
Felton, Frank Elliot, Esq., Cambridge, Mass.
Foote, John P., Esq., Cincinnati.
Force, Col. Peter, Washington.
Farrington, A. C., Newark, N. J.
Gold, Theodore F., West Cornwall, Conn.
Goldmark, Dr., Vienna.
Guildford, Hon. Mather, Cincinnati.
Harris, Chas. T., Esq., N. Y.
Hilgard, Julius E., Esq., U. S. Coast Survey.
Hillhouse, William, Esq., New Haven.
Hirzel, H., Lausanne.
Hiscox, Gardner D., New York.
Hitchcock, Ed., Jun., Amherst.
Holyoke, F. E., Esq., Cambridge.
Hough, Dr. Franklin B., Somerville, N.Y.
Hun, Dr. Thos., Albany, N. Y.
Hunton, Dr. Ariel, Hyde Park, Vt.
Ives, Dr. Eli, New Haven.
Ives, Dr. Nathan B., New Haven.
Jenner, Dr. Solomon, Esq., N. Y.
Jennings, N. R., Esq., New Orleans.
Johnson, S. W., Esq., New Haven.
Judd, Orange, Esq., New Haven.
Kempton, John, Esq., New York.
Kendall, Ed. Dwight, Esq., Cambridge, Mass.
Klipstein, Dr. Louis F., St. James, Santee, S. C.
Lyman, Chester L., Esq., New Haven.
McConihe, Hon. Isaac, Troy, N. Y.
McNaughten, Dr. Jas. T, Albany.
MacWhorter, Alex., Esq., New Haven.
Mansfield, Ed. D., Cincinnati.
March, Dr. Alden, Albany.
Marshall, Prof. Chas., Indiana.
Maverick, Augustus, Esq., New York.
Miles, Dr. Francis T., Charleston, S. C.
Mitchell, Maria, Nantucket.
Monson, Dr. Alfred, New Haven.
Morris, Margaretta H., Germantown, Pa.
Porter, Prof. Noah, New Haven.
Purcell, Rt. Rev. Bishop, Cincinnati.
Rainey, Dr. Thos., Cincinnati, Ohio.
Ray, Dr. Jos., Cincinnati.
Rice, Rev. N. L., Cincinnati.
Robb, Prof. Jas., M. D., Fredericton, N. B.
Robinson, Prof. H. N., Cincinnati.
Salisbury, Prof. Ed. H., New Haven.
Schaeffer, Peter W., Esq., Pottsville, Pa.
Schenk, Dr. J. Stilwell, Princeton, N. J.
Selden, Silas R., Esq., New Haven.
Spear, Charles V., Pittsfield, Mass.
Stadtmuller, Ludwig, Bristol, Conn.
Stebbins, Dr. Richard, Springfield, Mass.
Stetson, Charles, Cincinnati.
Townsend, Hon. Franklin, Albany.
Trumbull, Jas. H. Esq., Stonington.
Venable, Prof. Charles, Virginia.
Walker, Hon. Timothy, Cincinnati.
Warden, Dr. J., Cincinnati.
Weld, Mason C., New Haven.
Wells, Dr. Thos., New Haven.
Wetherill, Dr. Chas. M., Philadelphia.
Whitney, Eli, Esq., New Haven.
Wilcox, Lester, Esq., Canajoharie, N. Y.
Winchell, Alex., Esq., Eutaw, Ala.
Wing, Matthew G., Esq., Albany.
Wing, Dr. Joel, Albany.
Woodhull, Lieut. Maxwell, U. S. N.
Woolsey, Prof. Theodore D., New Haven.

Notitces of errors or omissions in the preceding lists should be communicated to the Permanent Secretary.

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Section of Chemistry and Mineralogy.

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General Meeting.

Section of Geology and Natural History.

Section of Chemistry and Mineralogy.

Section of Physics and Mathematics.

Fifth Day, Friday, Aug. 23, 1850.

General Meeting.

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Section of Physics and Mathematics.

Section of Geology and Natural History.

Section of Physics and Mathematics.

Section of Chemistry and Mineralogy.

Seventh Day, Monday, Aug. 26, 1850.

APPENDIX.

PROCEEDINGS

OF THE

NEW HAVEN MEETING, 1850.

FIRST DAY, MONDAY, AUGUST 19, 1850.

GENERAL MEETING.

The first session of the Fourth Meeting of the American Association for the Advancement of Science, was held in the Geological Lecture Room of Yale College, New Haven, at $2\frac{1}{2}$ P. M. Prof. Alexander Dallas Bache, President for the year 1850-51, took the Chair.

The list of Officers was then read, as follows:—

Prof. Alexander Dallas Bache, *President.*
Edward C. Herrick, Esq., *Secretary.*
Dr. Alfred L. Elwyn, *Treasurer.*

Standing Committee.

Prof. A. D. Bache,
E. C. Herrick,
Dr. A. L. Elwyn. } *Ex officio.*

Prof. Joseph Henry,
Prof E. N. Horsford.

Local Committee.

Rev. Dr. T. D. Woolsey,
Prof. D. Olmsted,
Prof. N. Porter,
William Hillhouse, Esq.
Prof. J. D. Dana,
A. N. Skinner, Esq.
Dr. John Knight,
Prof. E. E. Salisbury,
Prof. B. Silliman, Jr.

In compliance with the request of the Standing Committee, the meeting was opened with prayer by Rev. President WOOLSEY.

It was resolved that three Assistant Secretaries be appointed by the Chair. Prof. ELIAS LOOMIS, Dr. B. A. GOULD, jr , and WM. P. BLAKE, Esq., were accordingly chosen.

Prof. LOOMIS submitted a report of the proceedings of the Standing Committee at their meeting in the morning, with the list of papers to be read. Various gentlemen were then nominated by the Committee, and duly elected.

A communication was received from the Trustees of Amherst College, Mass., inviting the Association to hold its next annual meeting at Amherst, and to make use of the public rooms of the College for that purpose.

Professor O. M. MITCHEL stated that the Directors of the Astronomical Observatory at Cincinnati, Ohio, had entrusted him with a similar invitation to meet at that place, which he would present in writing at the morning session.

On motion, it was resolved that both invitations be referred to the Standing Committee.

A note was received from Prof. SALISBURY, inviting the members of the Association to his house on Tuesday evening, August 20. The invitation was accepted.

The first paper was then read by the author, as follows:

1. NOTES ON SOME POINTS OF ELECTRICAL THEORY. By DENNISON OLMSTED, Prof. of Natural Philosophy and Astronomy in Yale College.

I have from time to time made a few memoranda on certain subjects connected with the theory or practical applications of electricity, from which I propose to select a few points for the present paper.

1. *Phenomena produced by the Expansion of Air, when Buildings are struck by Lightning.* In the year 1843, a house was struck by lightning in the eastern part of the city of New Haven, where the effect in question first attracted my notice. The house had no lightning-rod, but the chimney, which rose from the centre of the roof, was the point of attack. A small fire was burning in a fireplace in the family room, the smoke of which undoubtedly invited the charge in that direction. The main charge

ran down the chimney as far as the roof, and taking a course of nails along one of the rafters, made its way to the tin gutter, and thence by the tin water-pipe found its way to the earth. But a fact of more interest was the indication, in the family room, of certain effects which were evidently due to the sudden and powerful expansion of the air of the chimney, and of the apartment below. The ashes and embers that were in the fireplace were blown out and scattered over the room, and the windows opposite the fireplace, at the distance perhaps of eighteen feet from it, were singularly broken, several of the panes of glass being cracked in a way which plainly denoted that it was done by an elastic force pressing uniformly from within; for each broken pane was divided into numerous sectors or triangles, by cracks diverging from a common point at the centre. In a few instances the sectors protruded outward from the central point.

Since my attention was first drawn to these facts, I have observed a number of instances, in inspecting houses struck by lightning, where similar indications were afforded, of a sudden and violent expansion of the internal air.

May we not refer to this cause some of those cases where heavy bodies are moved out of their places by a stroke of lightning? In a house in this city struck by a violent stroke a few years since, which tore its way through the house from the top of the chimney downward, heavy articles standing on the mantletree of the fireplace, such as lamps, vases, and the like, were overturned, and, in some instances, thrown to the distance of several feet. Were not these effects owing to the rush of air that was driven forward by the charge?*

2. *Effect of Mass in connection with a Conducting Surface.* Since electricity distributes itself at or near the surface of conductors, some have hastily concluded that it is of little importance whether a lightning-rod is a solid cylinder or a hollow tube; or whether, if solid, the interior part is a good or bad conductor, provided the surface is a good conductor. But the experiments of Faraday show that the conducting power of a rod is greatly affected by the nature of the mass, independently of the surface. Some facts have occasionally fallen under my notice, indicating the same truth. In examining the effects of a heavy charge of lightning upon the rooms of a house, which were greatly shattered, my attention was drawn to a looking-glass that was broken into numerous fragments. The lightning entered one corner of

* These effects are not to be confounded with those resulting from the repulsive energies of electricity. (See Arago, Annuaire for 1838, p. 336.)

the glass, and passed out diagonally at another corner; and, as the coating of the mirror exposed nearly six square feet of pretty heavy amalgam, it might have been supposed adequate for the transmission of the charge without resistance. But the numerous fractures produced in the glass showed that the conducting power of the metal was greatly impaired by its contact with the non-conductor. It may be inferred that sand by being wet is still but an imperfect conductor, and that hence a lightning-rod which terminates in a sandy soil has but an indifferent position, even when the sand is moist. In a silicious soil, therefore, it is peculiarly important that the bottom of the rod should be in communication with some better medium than the soil itself, such as a well, an aqueduct, an iron railway, or an iron fence. Not only in such a soil, but in most other cases, it will add to the security of a lightning-rod, to have points of delivery at the surface of the ground, even where the rod penetrates to a considerable depth below the surface; for the rain which usually accompanies thunder and lightning may greatly increase the conducting power of the surface before it has had time to penetrate to any depth. Metallic points projecting from the rod where it meets the top of the ground, or a rope made of fine twisted wires, connected with the rod at this point and laid loosely in contact with the ground, would probably add much to the security afforded by the rod.

3. *The Division of an Electrical Charge between different Conductors.* I have had several opportunities of observing the fact stated by Professor Henry, that even the best constructed rods, when transmitting heavy charges of electricity, readily part with portions of the charge to neighboring conductors. During the past summer, a house was struck with lightning in my own neighborhood, which showed the operation of this principle. The electricity found its way to a well in the kitchen, through the medium of a metallic pump, the shaft being of copper and the handle of iron. At the moment of the discharge, the cook was in the act of pumping water, having hold of the handle of the pump. With so perfect a conducting communication with the earth as was afforded by a large iron pump-handle, closely joined to a heavy copper shaft, which descended into a deep well, it might have been deemed quite safe to grasp the handle of the pump while the charge was passing; but the female was knocked down, and for a few moments rendered senseless.

Several instances of the tendency of a heavy charge of lightning to divide itself among different conductors, have fallen under my observa-

tion, where the ascending column of smoke from a chimney has come into competition with a lightning-rod attached to the same building. In one of these cases the rod was new, and constructed and attached in a manner that appeared unobjectionable. Moreover, it was evident from the discoloration of the gilt spindle, which formed the upper termination of the rod, that a heavy charge of electricity (much the greater portion of the whole shock) had in fact fallen on the rod, and been conveyed by it to the ground, preserving the inmates of this part of the house harmless. But, at the distance of about forty feet from the top of the rod, a current of smoke was ascending from the kitchen fireplace, which diverted to itself a small portion of the charge, that passed through the foot of a female who was at work near the fireplace, tearing the shoe on opposite sides, and the stocking between, and communicating to the foot a strong sensation of heat. Thence it passed through the floor, marking its way by a single sliver.

Since, during the season of thunder-storms, the kitchen chimney has usually a fire in it, and is the only one of the building that has a fire, this chimney evidently requires special protection; nor can it be considered as fully protected by a neighboring rod, even should it fall within the rule that a lightning rod will protect a space on all sides within double the distance of its height above the building.

In one instance, where a heavy stroke of lightning struck the chimney of a house, the charge seemed to descend unbroken to the garret floor, and there to divide itself into four streams, each pursuing a different route to the earth, and each strong enough to leave palpable traces of its path. The city of New Haven appears to be one of those places which are peculiarly liable to be struck with lightning. Seldom does a year pass without serious damage by lightning to one or more buildings: and in 1845, no less than seven dwelling-houses were struck, five of them with much violence. Three of the number were armed with lightning-rods; and although they did not furnish complete protection, yet they evidently transmitted, in each case, the greatest part of the charge, and probably were the means of preserving the lives of the inmates. The reason why this city is peculiarly subject to attacks from lightning, appears to be the nature of the soil, which is highly silicious, and consequently, in the dry weather that usually prevails in the season of thunder-storms, it presents to the conductor a strongly resisting medium. Small bodies of water, therefore, or spots habitually damp, invite an electric discharge; and the influence of such local circumstances in determining the direction of the charge, has been evinced

in two cases, where, within a short time, the lightning has struck twice in each place.

4. *Returning Stroke.* Among the numerous instances that occurred in 1843, of houses of public worship struck by lightning, two cases came to my knowledge where there were strong indications that the charge came from the earth. In both cases, (the one at Avon, and the other at Stamford, Connecticut,) the meeting-houses were furnished with rods, which appeared to have afforded partial but not entire protection to the buildings. From the base of each rod, a large quantity of mud was thrown up the rod, covering it as high as the eaves of the house. The popular impression was strong and universal, that the current of electricity came out of the earth; but the facts do not seem to me decisive on that point. Suppose the charge to have descended the rod; then on reaching the surface of the earth, wet as it was by a copious shower of rain, the electricity would escape freely from the rod, and the muddy surface of the earth around the base of the rod would become highly electrified. The particles of mud would thus be rendered mutually and strongly repulsive of each other, and tend to be dispersed on all sides, but having no freedom of motion in any other direction, they would appear to be only thrown upward. The effect appears to me analogous to that of a strong electric spark, as from a battery, upon a parcel of wafers, which are thrown upward, from whatever direction the spark happens to come.

5. *Trees struck by Lightning.* It is the popular impression in the southern part of the United States, that pine trees are of all trees the most apt to be struck by lightning, and that not merely because in certain districts pine forests are the most extensive of any, but even when mixed with other trees of the forest, the pine seems most frequently assailed, whereas its resinous character would lead us to expect for it an exemption from attacks of lightning. Is not this the fact—that the pine is probably not more liable to be struck than other trees, perhaps not so liable, but when struck, it exhibits greater marks of violence, in consequence of its non-conducting qualities? Since my attention was first drawn to this subject, some years ago, I have observed many facts in respect to the liability of trees to be struck with lightning, and I have been led to believe that this is a much more frequent occurrence than is generally supposed. Such an inference might be drawn from the number of lives, both of men and animals, that are every year destroyed by taking shelter under trees during a thunderstorm. In most cases where trees are full of sap, or are wet with rain,

they are such good conductors as to transmit the charge without receiving any marks of violence. In other cases, the sudden decay and death of trees, without any other apparent cause, suggests a probability that lightning may have been the true cause; and, finally, we may not unfrequently trace marks, more or less palpable, of the effects of electricity upon the trunks of trees. It has been a question of long standing, whether trees growing near a house afforded to it any degree of protection? Being usually in a state which renders them better conductors of electricity than the materials that compose buildings, and rising above the level of the buildings; moreover, presenting in their leaves and terminal branches numerous conducting points, they seem peculiarly adapted to act the part of natural lightning-rods; and I think the case has never happened where a charge of lightning, having first struck a tree near a house, has deserted it for the building, unless diverted towards the latter by some strongly conducting medium. I have known one instance where the lightning having struck a dwelling-house by way of a central chimney, and afterwards having made its way to the front side of the house, where a tall tree was standing, leaped to it from the eaves of the house, and thus found its way to the earth. I do not recollect the precise distance of the tree from the house, but think it could not have been less than five feet.

In the discussion which followed on Prof. Olmsted's papers,

Prof. Henry mentioned an instance of an explosion during the passage of an electrical discharge through a house, from which fact he had been led to the same conclusions on this point with Prof. Olmsted. He had made a series of experiments to ascertain whether the hypothesis was true or not. The results were attained by means of Kinnersley's Air Thermometer. The Professor's investigations convinced him that the effect was due to a sudden repulsive energy imparted to the air. He cited several instances, some of which were noticed by himself at Princeton, where the roof of one house was blown off, and the side of another blown out. He considered that the great mechanical effects of an electrical discharge are due in most cases to mechanical efforts as to the repulsive power of the air. He had made some interesting experiments in galvanism, whose effects he referred to the same cause.

The President stated that all the phenomena which he had read of, or observed in regard to the effect of the electrical shock on bodies,

would be explained by the high repulsion of the parts and by the action on the side of least resistance.

In regard to the distance at which the disturbance of electrical equilibrium took place, he had hoped that Professor Henry would have described the numerous experiments which he had made on the subject. In one case observed by himself, signs of fusion were exhibited in a theodolite placed upon a wooden stand, below a tripod used as a signal in the coast survey, the tripod having been struck by lightning. At the distance of an eighth of a mile, a man standing with his hand on a crowbar which was thrust into the ground, received a very severe shock.

The question as to what species of trees were most liable to be struck, was difficult to decide. In a journey made under circumstances favorable for such observations, he had noticed so many discrepancies and diversities in the facts, that several hypotheses which had successively been formed, had each in turn been found to be untenable.

The difficulty of inferring the direction of forces from a cursory examination of the directions in which objects had been thrown, was very great. A salute from Ringgold's battery, fired near the Capitol at Washington, had broken the glass in the windows, and the fragments had all fallen outward.

Prof. Loomis, in reply to the remarks of Prof. Olmsted, regarding the common impression of the South, that pine trees are not struck by lightning, had a few words to offer. He mentioned a similar important principle in Ohio, which is perhaps more general—that the beech trees are never struck. This was a prevalent idea, which it is quite dangerous to deny. But in one instance, to Prof. L.'s knowledge, a beech tree was struck by lightning. He considered that where trees are full of sap, they are in much less danger; when there is less sap, they are more frequently attacked. Dry trees are more sheltered, and retain the marks.

Prof. Johnson gave instances of the electric current describing a spiral course; following, as it were, the grain of the wood.

Prof. Brocklesby had had the pleasure, if pleasure it might be called, of being in the house at Avon when it was struck. The lightning apparently came down the rod, but this was improperly constructed. Instead of being continued into the earth for a considerable depth, its lower extremity entered the soil but four inches, from which point small pieces of pig-iron were laid, at distances of three feet, entirely detached

from each other—terminating at a hole of the size of a man's hat. The mud was thrown up to the height of the cornices, some twenty-five or thirty feet. Prof. B. also observed some remarkable appearances of scorched grass in the immediate vicinity; the electric fluid seemed to have divided itself into two streams. One of the burned paths was six inches wide. He was led to think that the lightning had left the rod and followed the water, which was pouring down at a projecting corner of the edifice.

Mr. W. C. REDFIELD stated one or two facts in corroboration of the theory of a division of the charge. In New York, in several instances, there had been apparently simultaneous discharges from the same cloud, taking different paths to the earth.

Dr. B. A. GOULD, of Cambridge, communicated the observations of an eminent engineer in Boston, Mr. Boyden, upon certain electrical discharges which occurred in Philadelphia during a severe thunder-storm, when a great number of houses were struck by lightning. In these cases the glass was cracked, and the majority of the broken panes had fallen outside. Dr. GOULD also gave instances of the spiral action of an electrical discharge, as mentioned by Prof. Johnson, and referred to the following letter to him from Prof. Goldschmidt, of Vienna:

"During the last summer, the lightning struck into the circuit between Brunswick and Hanover. I have informed myself of all the circumstances of the case, and it is perhaps not without interest for you to learn the details. At the distance of 0.7 of a mile from the Brunswick station, (a mile at 26,000 Brunswick feet,) the windows of the signal house were broken by the stroke. The pieces of glass all fell inward, the attendant fell senseless, and after he came to himself again, felt severe pains in his legs, which were very much swollen, and one arm was lame. He was cupped, and after a few days was quite well again. At the next house, 0.850 of a mile from the station, the attendant fell from his seat, and was speechless for half an hour. In the next house, the attendant suffered only the fright. He heard a loud rattling (*knattern*), and the building was filled with a smell of sulphur. The lightning struck the wire between the posts 0.792 and 0.794, and melted it. The wire was supported on poles about six feet high, which are provided at the top with a cap of porcelain, in which the wire (inclosed in a gutta percha tube) is fastened. The poles 0.80, 0.794, 0.792, were injured, inasmuch as great splinters were struck out in spiral lines, beginning a foot below the porcelain cap. One or two feet from the earth

the furrow ends. The spiral windings are from left to right, running about three times around the post. There are no trees or lofty objects in the neighborhood of the circuit where the lightning struck the wire on the posts. On the posts 0.796 and 0.798, there are no marks to be seen. The lightning also struck the circuit on the Thuringian road, and in the neighborhood of Frankfort-on-the-Maine, but no details of the subject are allowed to be made known, by order of the Prussian Government. It rained very little."

Prof. HENRY mentioned instances where ordinary electrical discharges had affected a circle of twenty miles in diameter. By an apparatus, simply constructed for the occasion, he had succeeded in magnetizing a needle by a flash of lightning so far off that he could not hear the thunder. He explained the apparatus. He considered that every flash of electricity produces effects to great distances, and perhaps affects half the globe.

The next paper read was entitled:

2. A COMPARISON OF THE FACE OF FISHES, WITH THAT OF OTHER VERTEBRATA AND MAN. BY PROF. AGASSIZ.

[*Not received.*]

Prof. BENJAMIN SILLIMAN, Jr., presented an oral communication, as follows:

3. ON THE ORIGIN OF A CURIOUS SPHEROIDAL STRUCTURE IN CERTAIN SEDIMENTARY ROCKS.

Prof. SILLIMAN said that the subject of his communication was simple. All acquainted with the geology of New York were, he said, familiar with the specimens of ——— from the Niagara group, and especially from the vicinity of Lockport. The specimens exhibited presented a curious embossed and concave appearance. The concavities were sometimes opposite to the convexities; they were not, however, opposite to each other in all directions. Prof. S. exhibited casts taken by Mr. Manross, and showed that similar appearances are produced at the present day by natural causes. It was noticed as somewhat remarkable, that

the regularity of the arrangement of these impressions was such that, by taking any one as a centre, six others may pretty generally be counted immediately around it. The appearances occur in non-fossiliferous sedimentary rocks. They have not escaped the attention of New York geologists, Mr. Hall having figured some specimens on p. 93 of his Report upon the Fourth Section of the Survey. The probable cause of the phenomena has been a matter of considerable curiosity with geologists. The causes producing like effects at the present day were stated by Professor SILLIMAN. He had found that excavations are made by tadpoles, identical in appearance with the older deposits of the Sedimentary formation. But there are no batrachians so old as the age of the Niagara group. The cause of the phenomena was attributed, in this dilemma, to the agency of great numbers of small gregarious fishes, and other animals, also gregarious, which are known to have existed at that period.

He here presented a letter from Mr. Manross, as follows:—

NEW HAVEN, *August* 15, 1850.

PROF. B. SILLIMAN, JR.

Respected Sir:—The cast marked No. 1 was taken from a slab of blue limestone, marked as found at Erie, near Lockport, N. Y. It is now in the possession of W. S. Clark, of East Hampton, Mass. The slab is about ten inches in length and breadth, and is split into two layers about ¾ inch in thickness. All the broad surfaces present the same kind of markings—the upper is covered with depressions, the middle joint fitting, as in the cast, and the lower exhibiting protuberances. Those above, however, do not necessarily correspond in vertical position with those below, showing that they were not produced by any force or pressure acting through several layers at once, as in the case of footmarks.

The cast marked No. 2 was taken in the surface of mud now in progress of deposition. The locality is Bristol, Conn. Several square yards of the bottom of the pool from which the mud was taken, were covered with those curious depressions, presenting much the appearance of the indented surface of a thimble. By observing the motions of a swarm of small tadpoles in the pool, the cause of the depressions was easily seen. Whenever the little animals moved from one part to another, they almost invariably stopped in one of these hollows; and an occasional vibration while there, with the few rapid strokes in starting, deepened the hollow at every new visit. On examining both of the casts, it will be seen that the markings occur with similar and surprising

regularity. Taking any one for a centre, as in the cells of the honeycomb, six will be found adjoining it. It seems to be only another and, considering their origin, an unexpected example of the rule for economy of surface.

Not knowing whether these observations may be new or not, I submit them for your inspection.

Yours respectfully, N. S. MANROSS.

Professor AGASSIZ remarked that he could not at first conceive how an animal with the peculiar form and appendage of the tadpole could produce such a concavity; but, after reflection, he would not say that he believed it impossible.

Prof. ADAMS inquired, if the raised parts were turned upward, or if they were below the plane of the surface, in the Lockport rock.

Prof. SILLIMAN replied, that not having visited the locality, he could not so well state the exact position as his friend, Prof. Horsford, who had been there.

Prof. HORSFORD thought that the cavities were depressed; and, therefore, the explanation of the production of the cavities in the mud at Bristol by tadpoles, would apply here also.

Prof. H. continued, and stated that he had, in company with Prof. Agassiz, observed the production of raised hemispherical surfaces in the mud near Cambridge, by the rising of gases from decomposing organic bodies. He referred also to the depressions caused by rain-drops.

Prof. SILLIMAN reminded the Association, that the tadpoles were certainly producing cavities in Bristol.

Prof. HENRY spoke of the surfaces left by rain-drops, and remarked, that water, falling from an elevation through air, took the form, not of a sphere, but was flattened, or hollowed out underneath, and assumed a form somewhat like a parachute. Such masses of water, in their contraction, drew the dust or earth upon which they fell into heaps. Prof. Henry had verified this by experiment.

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4. ON ELECTRICAL PHENOMENA OBSERVED IN CERTAIN HOUSES. By PROF. E. LOOMIS.

The Professor remarked, that within the past few years, several houses in the city of New York have exhibited electrical phenomena in

a very remarkable degree. For months in succession they have emitted sparks of considerable intensity, accompanied by a loud snap. A stranger, upon entering one of these electrical houses, in attempting to shake hands with the inmates, receives a shock, which is quite noticeable, and somewhat unpleasant. Ladies, in attempting to kiss each other, are saluted by a spark. A spark is perceived whenever the hand is brought near to the knob of a door, the gilded frame of a mirror, the gas-pipes, or any metallic body, especially when this body communicates freely with the earth. In one house, which I have had the opportunity to examine, a child, in taking hold of the knob of a door, received so severe a shock that it ran off in great fright. The lady of the house, in approaching the speaking-tube to give orders to the servants, received a very unpleasant shock in the mouth, and was very much annoyed by the electricity, until she learned first to touch the tube with her finger. In passing from one parlor to the other, if she chanced to step upon the brass plate which served as a slide for the folding doors, she received an unpleasant shock in the foot. When she touched her finger to the chandelier, (the room was lighted with gas by a chandelier suspended from the ceiling,) there appeared a brilliant spark and a snap. In many houses the phenomena have been so remarkable as to occasion general surprise and almost alarm. After a careful examination of several cases of this kind, I have come to the conclusion that the electricity is created by the friction of the shoes of the inmates upon the carpets of the house. I have found, by direct experiment, that electricity is excited by the friction of leather upon woollen cloth. For this purpose, I stood upon an insulating stool, and, spreading a small piece of carpeting upon a table before me, rubbed a piece of leather vigorously upon it; and then, bringing the leather near the cap of a gold-leaf electrometer, the leaves were repelled with great violence. The electricity of the leather was of the resinous kind. Electricity, therefore, must necessarily be excited whenever a person walks with a shuffling motion across a carpet; but it may be thought remarkable, that the electricity should be intense enough to give a bright spark. In order to produce this effect, there must be a combination of several favorable circumstances. The carpet, or at least its upper surface, must be entirely of wool, and of a close texture, in order to furnish an abundance of electricity. So far as I have had opportunity to judge, I infer that heavy velvet carpets answer this purpose best. Two thicknesses of ingrain carpeting answer very well. A drugget, spread upon an ingrain carpet, yields a good supply of the fluid. The

effect of the increased thickness is obviously to improve the insulation of the carpet. The carpet must be quite dry, and also the floor of the room, so that the fluid may not be conveyed away as soon as it is excited. This will not generally be the case except in winter, and in rooms which are habitually kept quite warm. The most remarkable cases which I have heard of in New York have been of close, well-built houses, kept very warm by furnaces; and the electricity was most abundant in very cold weather. In warm weather only feeble signs of electricity are obtained. The rubber, viz., the shoe, must also be dry, like the carpet, and it must be rubbed upon the carpet somewhat vigorously. By skipping once or twice across a room with a shuffling motion of the feet, a person becomes highly charged, and then, upon bringing the knuckle near to any metallic body, particularly if it have good communication with the earth, a bright spark passes. In almost any room which is furnished with a woollen carpet, and is kept tolerably warm, a spark may thus be obtained in winter; but in some rooms the insulation is so good, and the carpets are so electrical, that it is impossible to walk across the floor without exciting sufficient electricity to give a spark. It may be said, that, in this case, there can be but very little friction between the shoe and carpet. But it must be remembered that the rubber is applied to the carpet with considerable force, being aided by the whole weight of the body, so that a slight shuffling of the feet acts with great energy. In the *London and Edinburgh Philosophical Magazine*, for February, 1839, is given an account of a leather strap, connecting the drums of a worsted mill, which gave sparks two inches in length, and charged a battery in a short time. The strap was twenty-four feet long, six inches broad, and one-eighth of an inch thick. It crossed in the middle, between the two drums, the strap forming a figure eight. Here there was considerable friction, since the straps made one hundred revolutions in a minute. In the *American Journal of Science*, for July, 1840, is mentioned an instance of a leather band in a cotton factory, which exhibited decided electrical excitement. These examples show that leather, when subjected to considerable friction, yields an abundant supply of electricity. In the proceedings of the American Philosophical Society, for December, 1840, are mentioned several cases of individuals who drew sparks of electricity from a coal stove and from a common grate. I consider it probable that, in these cases, the experimenter was the electrified body, and not the stove or grate. How is it possible for a grate, containing burning coals, to be insulated so as to retain a charge of electricity? On the

other hand, it is presumed that the experimenter was insulated by standing upon a carpet made quite dry by a winter fire.

Prof. Silliman referred to the case of an electrical lady, in New Hampshire, described in the *Journal of Science.*

A communication was received from Prof. C. U. Shepard, inviting the members of the Association to his house, on Wednesday evening next, and a similar one for Thursday evening, from Hon. A. N. Skinner, Mayor of the city. Both invitations were accepted, and the meeting adjourned to meet at 9, A. M., on the 20th.

SECOND DAY, TUESDAY, AUGUST 20, 1850.

GENERAL MEETING.

(Morning Session.)

The Association met in the Geological Lecture Room at 9 A. M., when the minutes of Monday's session were read and adopted.

The President called the attention of the members to the rules requiring abstracts of communications to be given to the Secretary, and desired that it might be complied with.

Mr. J. V. L. Pruyn, of Albany, presented communications from the Regents of the New York University and from the Albany Lyceum, requesting that the next annual meetings of the Association might be held in the city of Albany. The invitation was referred to the Standing Committee.

In accordance with the recommendation of the Standing Committee, the following additional members of that body were appointed;

Prof. H. D. Rogers, Prof. L. Agassiz, Prof. B. Silliman, jr., Prof. D. Olmsted, Prof. J. P. Norton, Wm. C. Redfield, Esq.

Prof. Henry D. Rogers reported from the Committee on Fees and Tickets, recommending the appointment of a Secretary, who shall hold his office three years, and receive a salary of \$300; and also that the assessment upon individual members, in addition to the annual payment, be \$3. This communication was referred to the Standing Committee.

Dr. B. A. Gould, jr., was excused from serving as one of the assistant Secretaries, and the President was authorized to fill the vacancy thus created.

In compliance with the recommendation of the Standing Committee, it was voted to divide the meeting into two Sections, viz.: one of *Geology and Natural History,* and one of *Physics, Mathematics, and Chemistry.* The General Meeting then adjourned, and the two Sections continued in session, in separate rooms, during the remainder of the morning.

SECOND DAY, TUESDAY, AUGUST 20, 1850.

GENERAL MEETING.

(*Afternoon Session.*)

The Association convened in general session, at 3 P. M. in the College Chapel.

The Standing Committee to whom was referred the communication received in the morning from the Committee on Fees and Tickets, presented, through Professor Loomis, a report, recommending the proposals comprised therein. Whereupon it was

Voted, that an assessment of three dollars be paid to the Treasurer by each member of the Association in attendance.

It was also voted, that a Secretary be appointed, who shall hold his office for the term of three years, and shall have a salary of $300 per annum, and whose duty it shall be to compile for publication all proceedings or transactions of the Association, to superintend the publication of the same, and to conduct the correspondence; the title of said officer to be that of Permanent Secretary to the American Association.

Professor Henry then made some remarks on the Measurement, Origin, and Classification of Mechanical Powers.

[*Not received.*]

This communication gave rise to an extended discussion, in which Prof. Coffin, Prof. W. B. Rogers, Prof. Henry, and Mr. Allen took part.

Prof. Rogers referred to the writings of the late Prof. Robinson, of Edinburgh, who, as he believed, had first set forth in a clear and philosophical light the origin of the difference, and had shown that, properly understood, the two measures are entirely coincident. Prof. R. then proceeded to explain the views which, in accordance with Prof. Robinson's exposition, he had long been accustomed to maintain and

teach. He illustrated the difference between the two modes of measuring power, by the example of an arrow, shot vertically upwards, and a cannon ball discharged against a mass of homogeneous wax or wood. The arrow, when discharged with a double velocity, will ascend to a quadruple height, and a cannon ball, shot with a double velocity against a mass of homogeneous wax or wood, will penetrate to a quadruple depth. In both cases it is to be considered that the mass, moving with a double velocity, will require the resistance to be continued for a double time, in order that it may be brought to rest. Hence, having a double velocity, and a double time of motion, the space described must be quadruple. Thus, in both cases the work done, that is, the product of the mass into the space described, varies as the square of the velocity.

Professor R., referring to the use of the term power, in mechanical inquiries, objected to the conception so usual among physicists of the *transferrability* of power. This involves the idea of power as a distinct entity, something separable from one mass, and capable, as it were, of being passed over to another. Thus, a body in motion, striking a body at rest, and causing it to move, is said to transfer its motion to the other. But as power and motion are but conditions of active matter, it seems unphilosophical to speak of their being conveyed from one body to another. Power may be excited or brought into play by the approach of one body to within a certain distance of another, and motion thus be caused by the mutual repulsion of the proximate parts; but in this case the diminished motion of the striking body, and the acquired motion of that previously quiescent, are but the results of this mutual repulsion, and the moving body cannot be considered as transferring either motion or power to the other.

Dr. Charles G. Page then made a communication, illustrated by experiments, as follows:

On Electro-magnetism as a Motive Force.

After mentioning the plans hitherto proposed for the application of electro-magnetism as a moving power, and the reasons of their failure, he submitted an account of his own experiments and of the arrangements he had employed, and stated that he had succeeded in constructing an engine of four-horse power, which he believed could be maintained at a cost of twenty cents per day (24 hours) for one horse power. The great advantages of his method of magnetization and arrangement of the helices, consisted in the economy of electric current

and the action of the force continuously through a great distance. The average force acting upon the magnetized bar in his experiments he stated to be 300 lbs., and in some cases equalled 600. Professor Page also stated the interesting fact, that when the circuit was broken near the magnet (after having been closed for a few seconds), the reports were as loud as those from a pistol, becoming less violent as the break was removed to a greater distance; the secondary spark causing the report diminishing from eight inches to one-sixteenth of an inch.

This communication elicited remarks from Prof. H. D. Rogers, Prof. Johnson, Mr. Allen, Prof. Henry, Prof. Silliman, and Prof. Peirce.

After the reading of the programme for Wednesday, the meeting adjourned.

SECOND DAY, AUGUST 20, 1850.

(*Continued.*)

SECTION OF GEOLOGY AND NATURAL HISTORY.

The Section of Geology and Natural History was organized at 10 A. M., by the election of Prof. H. D. Rogers as Chairman, and of Prof. O. P. Hubbard and Dr. W. J. Burnett, as Secretaries.

The following communications were then presented:—

On the Growth of the Egg, prior to the Development of the Embryo. By Prof. L. Agassiz.

When Schwann showed that all development took place from cells, it was immediately inferred that everything arose from a single cell originally, but this was not proved. I have had the good fortune to make some observations in this direction, and to trace with grasshoppers the single cell up to the ovum; so that now I consider it an established fact, that the ovum is primitively a true cell. It is, therefore, of much importance to trace the variations of this cell up to the new being. This department of embryology has been much less studied than others, and here is an extensive field for the investigator. The ovum grows much before the germ appears; this is an important point, and this growth varies exceedingly in different parts of the animal kingdom. It should be understood that the external portions of the egg are mere accessories, protecting forms, while the yolk is the essential part, as we see with birds. Among the lower animals, however, as, for instance, among the

Mollusca, the eggs are not of uniform size throughout the same class; thus the tropical genus Bulimus, which is of itself no larger than a turkey's egg, lays eggs as large as a pigeon's egg; but in the whole class of birds the eggs are large. In reptiles, again, two divisions exist. In many, the egg is always small prior to the appearance of the germ; but in others it is very large, and has external coverings, as in the turtle for instance. What are the changes of this primitive cell? and how is it that it increases? It is by the development of cells within itself. Are these cells developed from granules? This is what might be supposed, but the nature of these granules I will defer speaking of, at present, as I believe that special information on this point will be laid before the Association by another. In the egg of mammalia we have the vitellus, the germinative vesicle within it, and within the latter germinative dots; the number of these dots I now regard as of no value, as they may be from one to six or seven, and are nothing but granules; the vitellus enlarges by the dilatation of the yolk vesicles from endosmosis, which are nothing but simple sacs filled with an oily fluid. But these vitelline cells differ as they are at different distances from the germ; but to ascertain these differences of cells is a matter of great difficulty, and which I could discover only by boiling the egg, which process did not destroy the differences. As the ovum advances in age, these differences become apparent: the cells are smaller as you approach the periphery, and at it are granules, so that there is a third kind of cells in the mature egg which are more tenacious than the rest, so that in making a section of the egg you have three layers marked by three different colors—dark at the centre and periphery, and light yellow in the middle. This outer layer is the blastoderma, which is not a living substance, but is the basis of the future animal, for as the changes go on, it forms the cicatricula. The yolk cannot be considered as a mere collection of food for the new animal. It is the animal itself in an amorphous condition, and its various parts soon appear out of it; and that it is an animal is fully proved by the fact that it moves by ciliæ situated on the external layer of cells.

Prof. H. D. Rogers asked if some remarks would not be made upon the similarity of the seeds of vegetables and ova.

Prof. Agassiz remarked that the analogy was complete, and the ova of many plants so exactly resembled the ova of many animals, that they have been described as animals. He also stated that these same ciliated cells from the vitellus were often described as infusoriæ, and it was frequently a matter of great difficulty to decide the difference.

Some Observations on the Gold Formations of Maryland, Virginia, and North Carolina. By Prof. W. R. Johnson.

The belt or district of country in which occur the gold-bearing rocks of the Atlantic border of the United States, appears to range longitudinally from north-east to south-west in a general direction, not far from N. 32 degrees E. This direction is the result of a great number of observations taken in all the three States, and at points where the formation appears to be the most regular and determinate.

It also results from a general observation of the relative position of the extreme points at which the central axis of the gold district has been noticed. Taking Brookeville in Maryland, and tracing by Rockville to the point of crossing of the Potomac below the Great Falls, extending thence across the Rappahannock, ten miles above Fredericksburg, thence through Stafford, Fauquier, Culpepper, Spotsylvania, Orange, Louisa, Fluvanna, Buckingham, Campbell, and Pittsylvania, in the State of Virginia; through Rockingham, Guilford, Davidson, Rowan, Cabarrus, and Mecklenburg counties, in North Carolina. By prolonging the same axis north-eastwardly, it passes through a part of York county, in Pennsylvania, in which gold is said to have been detected; and several hundred miles further to the north-east, it strikes the town of Somerset, in Vermont, in which, according to Prof. Hitchcock, gold was also discovered more than thirty years ago.

As the result of special observations on the strike of the slate beds, in which the gold veins occur injected between the plies of sedimentary rocks, the facts observed were found between Rockville and Brookville, in Maryland, where the bearing is N. 30 E.; on the borders of Spotsylvania and Orange counties, in Virginia, N. 29 to 32 E.; in Montgomery county, North Carolina, at the Russel mine, N. 32 E.; and in Mecklenburg county, at the Smart mine, twenty miles south-eastward of Charlotte, the strike of the beds being there N. 32 E. These are a few of the points noticed, and the results are obtained from numerous observations taken at each point.

The system of metamorphic rocks, in which the gold-bearing veins occur, appears to have undergone different degrees of change in the different parts of the tract. While in some parts the original slaty structure is preserved, in others the lamination has been partly obliterated, and the texture changed by the evident effect of heat. In some points to which observation has extended, there is evidently an intermingling of rocks of the gneissoid character with such as still retain the

slaty structure. In certain parts of the North Carolina gold region, the granitic rocks prevail, and there the auriferous veins have various directions, apparently wholly irrespective of the general trend of the gold formation. Thus, between the town of Charlotte and the Catawba river, and within a circuit of about three or four miles in diameter, are found veins which have been more or less extensively worked, with directions running to the N. 64 E., N. 47 E., N. 8½ W., N. 26½ E., and N. 34 W., so that if these directions were prolonged they would, in some cases, be found intersecting each other nearly at right angles.

In regard to the materials, or veinstone, in which the auriferous particles are found, they differ very widely; in some cases the material is an argillo-talcose slate, of a silky lustre, much interspersed with minute cubes of pyrites of iron or of copper, or both, as at the Russel mine on the Uwharry, in Montgomery county, in North Carolina. In other cases it is partly in quartz, and partly in the slaty walls of the veins; and in others still it is wholly in the quartz, the walls being scarcely at all impregnated with the precious metal.

The materials which exist in the veins are either such as have been acted on by meteoric influences, and partly decomposed, or, lying beyond the reach of such influences, have escaped decomposition, and may be regarded as the true exponents of the deeper veinstone. These latter are in general less rich in gold than the former, chiefly on account of having lost a part of their solid material by decomposition.

But the deep ores owe their inferior value in no small degree, also, to the difficulty of extracting the gold, from its combination with the sulphurets, which, near the surface, have been reduced by the combined action of air, water, and other materials from the atmosphere.

Prof. W. B. Rogers followed Mr. Johnson, in some observations upon the geological position of the auriferous belt in the United States, and upon the conditions under which the gold is found in the veins at the surface and at considerable depths. He stated that the general direction of the auriferous beds corresponds to that of the old metamorphic rocks with which they are associated. The quartz veins usually run parallel with the bedding of the adjacent strata, but occasionally in an obliquely transverse direction—often they are single, but sometimes ramifying. It is evident that the great mass of this igneous material rose to the surface between the dividing planes of the talcose and micaceous slates in which they occur. Prof. R. called especial attention to the very different condition in which the gold is found in the super-

ficial parts of the vein, and at depths below the reach of meteoric agencies. Near, and at the surface, the quartz is cavernous, exhibiting the cavities formerly occupied by the sulphuret of iron, with which the gold was intimately blended. In them are frequently found granules and spangles of gold; but the sulphuret of iron has been decomposed and removed. The resulting oxide of iron is found collected along the sides or walls of the vein, forming sometimes valuable beds of iron ore, while much of the gold is left in grains, or small segregated masses in the body of the quartz. In this condition, its separation is comparatively easy, nature having already removed the sulphuret of iron, the ingredient which retains the gold with most tenacity under the ordinary purifying processes. Prof. R. urged the importance of keeping in view this difference between the asssociations of the gold near the surface of the vein, and at considerable depths, as it plainly indicates that the actual productiveness of the vein-mines might be expected to diminish after reaching some depth below the surface, even while the real amount of gold present in the rock would probably be as great, or greater, below than at the surface.

Researches on the Origin, Development, and Nature of the Spermatic Particles throughout the Vertebrata. By Dr. W. J. Burnett, of Boston.

[Abstract.]

Of the Testes. The testes, the eliminating organs of the semen, are essentially the same throughout the four grand classes, and consist of a basement tissue, on which rest the immediate secreting organs of the Spermatic Particles: this is a layer of cells. Upon the various modes of the packing together of this basement tissue and cells depend the various forms of the testis—its size, prolific power, &c., &c. Previous to the animal's age of puberty, these secreting cells have all the characteristics of the *epithelial cells* lining other glands; but, at the advent of the time of procreation, they pass on to a higher condition of existence, which consists in their power of eliminating these peculiar fecundating bodies; they are then called *parent sperm-cells*, which, therefore, are only modified epithelial cells.

The Spermatic Particles are formed only within these parent sperm-cells.

Of their Formation. The epithelial cell, nucleated and nucleolated, at

the time of the sexual impulses, rapidly increases in size, and seems endowed with a new life. When a certain size has been reached, the nucleus begins to segment, dividing into two equal spheres, commencing by a slight sulcus on one surface, which gradually deepens until the division is complete. Each of these divided spheres again divides, and this fissuration goes on until the whole is a mulberry-looking mass; and if the process has not been imperfect, the result in numbers is always *even*, since each division produces double the number of the last—thus: 1—2—4—8—16—32—64, &c., &c. Each one of these *ultimate* spheres (that is, those of the last subdivisions) forms a spermatic particle, and this occurs according to two methods particularly, the real existence of others not being positively settled.

The first of these is by an apparent conversion of the *whole* sphere into the particle, its shape gradually changing from that of a sphere to pyriform, and then candate.

The second is by a change of the *nucleus* of the sphere into the particle, the cell-membrane then being cast off.

The other apparent modes of formation which are occasionally seen need not be mentioned, as they cannot be satisfactorily demonstrated.

These remarks refer only to the production of the body, or so-called *cephalic portion*, of the particle.

As to the genesis of the *tail*, it appears to occur in a manner quite different, being the result of an arrangement of *granules* upon the extreme point of the cephalic portion, in a linear series.

The observations and experiments leading to this conclusion cannot here be described. It may be sufficient to say, that the tail holds a close relation with the vitality of the particle, and its perfect character, length, and tenacity, is a good index of its efficiency.

Now, it has been satisfactorily shown that the ovum is primitively a simple cell; in fact, an epithelial cell, originating on the inner surface of the ovarium, exactly like the corresponding cell on the inner surface of the tubes of the testis. The ovum and the parent sperm-cell correspond, therefore, in their identity of origin—both primitively a simple cell. Each has its function to perform, and this is far above the agency of ordinary cells; and we will now see if this correspondence is there carried out.

The segmentation of the ovum, or, rather, of the vitellus, has long been an established fact. It has been supposed to be the first expression of its fecundation, since it always occurs in a more or less marked degree after that process; but more extended inquiries of late have shown, that, although intimately connected with this vitalizing act, it

is, nevertheless, not dependent upon it, since it occurs in fishes before it can have been performed.

This segmentation of the vitellus is a multiplication of its parts, which are always spheres, by a repeated halving, and this goes on until the whole vitellus is reduced to a mulberry mass—the divisions producing simply the double of the last—thus: 1—2—4—8—16—32—64, &c., &c. Now, what is the object of this process? It appears to be a series of acts, by which the vitellus is vitalized; and the ultimate object of these spheres is, by their aggregation, to form the new being.

In the *unimpregnated* ovum this segmentation, when it occurs, is very limited; and I do not recollect of seeing the vitellus divided into a greater number than four parts. It here appears to have no end or object, and may, perhaps, be regarded only as an expression of that vitality which, had it been urged and directed by the act of fecundation, would produce a new being. In segmentation, therefore, we have the first, yet highest material expressions of vitalization. In the ovum, we have seen, that the *whole* result goes to form a *single* organism, the *embryo;* but, in the sperm-cell, *each* result (a single sphere) goes to form a single organism—a *spermatic particle.* The analogy, therefore, of this sperm-cell and the ovum is complete. The one is the active, the other the passive, part,—one fecundating, the other fecundated.

With the male, the vitalizing processes occur and fulfill their end by the inherent power of the animal; with the female, however, in the ovum, this power seems much more contracted; they require the directive and combined influence of those of the male to fulfill the end of all sex. Each spermatic particle is a representative of the whole, and its contact with the ovum is only required to communicate the same influence by which it was itself elaborated, viz., vitalization by continual segmentation.

The grand result attained by these reasearches, then, is an attempt to show the complete identity of nature of the male and female products for reproduction—the sperm-cell and the ovum, both of the same origin, and what leads to the elimination of spermatic particles with the one, leads to the elimination of the embryo in the other.

The unity of idea here displayed cannot fail to strike every one with feelings of admiration. Formerly it was supposed that the process of segmentation of the vitellus was unique and peculiar to the ovum, and that the function of the female was quite dissimilar to that of the male. But here we see that one is only the complement of the other; that each is a part of one grand process; and that, when they find their

perfect union, manifestations of life—the highest end of material forms—are produced.

ON RUTILE AND CHLORITE IN QUARTZ. By Prof. O. P. HUBBARD.

Specimens of rutile in quartz have, for twenty years past, been found in boulders in several towns in the vicinity of Dartmouth College, none of which have ever been traced to their sources. Localities have been mentioned, but none have furnished specimens resembling these boulders, excepting a single one. This locality was opened two years since at Waterbury, Vt., on the Central Railroad. It was described by Mr. Alger, in the Proceedings of the American Association for 1849, and also in the *American Journal*, vol. x. p. 14.

In a cut of sixty feet perpendicular through solid talcose slate, and thirty feet from the surface, a vein or pocket of quartz was met, and a considerable number of specimens containing rutile were obtained. The locality is now exhausted. From its position, it never could have furnished the scattered masses heretofore known, and we have yet to discover their origin.

Some of the specimens from this region have comparatively but little beauty; the rutile is in very fine capillary crystals of dark color, two or three inches in length, and the quartz is of inferior quality. But others are exceedingly fine, both in the richness of the quartz and the abundant long needles of the rutile.

There are *three* known American specimens of a remarkable character, one of which is from this Waterbury locality. The other two were found as boulders, and are even of superior quality.

One of these has been in the cabinet of Dr. J. R. Chilton, New York City, for many years, and is reported to have been found in Northern New England. It has the rutile in long acicular crystals, and one series of prisms united into a crystal a quarter of an inch wide.

The other is a mass in the writer's cabinet, described by Mr. Alger as "the finest specimen of this mineral found in the United States." It was picked up in this region nearly twenty years ago, but in what town is not known. Specimens from Rochester and Bethel, Vt., resemble it more than any others.

It is about six inches long and three inches in its other dimensions, being of irregular shape, and only a fragment of a larger mass. Two

sides have been cut and polished by the lapidary ; one retains its polished plane boulder surface, and the remaining part of the exterior is irregular, presenting a conchoidal fractured surface. There are indications of smooth cleavage faces in different parts, inclined to each other.

The quartz in mass is transparent and slightly smoky, while the slices cut off are almost colorless. It is questionable whether the color is proper to the quartz, or occasioned by the reflection from the rutile crystals. Mr. Alger finds almost no rutile in the *white* quartz crystals from Waterbury, " while the colored varieties abound with it," and probably, he suggests, owe their color to it.

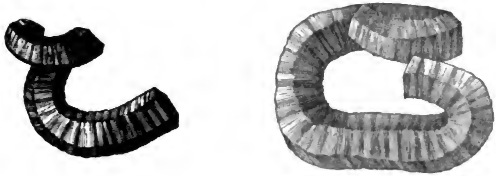
The rutile crystals are from the size of the finest hair, and almost invisible, up to a twelfth of an inch in diameter and five inches long; they are uniformly distributed through the quartz, and intersect and cross each other in all directions. There is no radiation from a centre; but, in many instances, the crystals have one or more large graceful curves, and sometimes two, in opposite directions; and some are bent at an angle either right or oblique. Many are broken at the surface of the quartz, while others are wholly included in it, terminating in a single plane, or tapering to a point.

They are all of a uniform bright, reddish-brown color, and of the lustre of polished copper. Where the ends are seen on the polished faces, they have the color and lustre of polished steel.

In numerous cases, the surface of the crystals is covered here and there with a brilliant silver-white mineral, sometimes limited to the lateral edges, and again investing parts of the prism at intervals, or with frequent interruptions, giving it the appearance of being made up of numerous short white and brown prisms, the form remaining unchanged. In some cases this mineral occurs like a thin disk, through the centre of which the rutile appears to penetrate. I have not been able to determine with certainty the nature of this mineral, and can only conjecture that it is the same with the curved crystals described below.

In the author's specimen, as in those described by Mr. Alger, there are numerous vermiform, tortuous, and convoluted crystals. By transmitted light, they are sometimes of the color of copper, though faintly so, or of a bronze yellow, or of greenish and yellow shades, or even very dark, and by direct light they are almost black. These crystals are regular hexagonal prisms, transversely finely striated, and appear to be made up of thin plates, of slightly varying size, giving the crystals a varying diameter. They occur either singly, or in groups of several, laterally joined, and united in all their convolutions, and having a single

terminal plane, highly lustrous, which often presents a silver-white color. The subjoined figures, enlarged views of two of them, give a perfect idea of the originals, the prismatic form of which is obvious to the eye, and perfectly distinct with a glass. If we judge from the figures in Mr. Alger's paper, above referred to, the prismatic character of his crystals is much less striking.



Mr. Alger has described the mineral in his specimens as *mica.* I have been able to obtain only a very small portion of the mineral from one or two protruding curves on my specimen. It readily cleaves parallel to the terminal plane, is apparently softer than mica, and is easily reduced, by the pressure of a knife on white paper, into a fine, coherent powder, of a greenish tint. It has no elasticity, and, before the blowpipe, gives off an abundance of water. From these decided characters, and the rarity of such an association of mica, and the quite frequent one of chlorite and quartz, it seems altogether probable that this mineral is *chlorite.*

If these several minerals were at one time in solution in the fluid quartz, they must have crystallized previous to it. The rutile prisms are so straight, or so gracefully curved and bent, that they would seem to have experienced but slight resistance. They intersect and cross each other, and pass *through the loops in the chlorite* crystals, or touch them on the outside, and they probably crystallized first. Around most of these convolutions of *chlorite* there is a burr, or minute spot of imperfectly radiating fractures, occasionally iridescent, which suggests that they were formed before the solidification of the quartz, and that they had occasioned some pressure or disturbance, and a slight fracture. But as the chlorite uniformly, and the rutile in very many cases, must have been without any attachment, the density of the fluid quartz to have sustained them was probably great.

On removal of the rutile and chlorite from the gangue, the vertical striæ of the former, and the transverse striæ of the latter, are found

figured on the quartz, making it certain that the latter was last solidified.

There must be somewhere in this region north a rich deposit, for which mineralogists will earnestly seek until it is found, and its treasures are transferred to their cabinets.

Prof. HUBBARD then exhibited to the Section a cast of a very perfect fossil elephant's tooth, found in a bed of gravel in Homer, N. Y., a few years since. The length of the crown is 10 inches, width $3\frac{1}{2}$ inches, and the longest diagonal 13 inches.

Prof. AGASSIZ remarked this was one of the most interesting specimens he had ever seen, and though so large, was not yet fully developed, as about two inches of the crown was not fully expanded or elevated so as to become a part of the grinding surface. He spoke of the great necessity of an entire revision of the subject of fossil elephants.

There were undoubtedly many species made on insufficient data, and the differences in the teeth arising from sex and age had not been sufficiently studied and determined. It was obvious, that different species formerly pervaded the southern portion of the United States from those found in the northern.

Prof. AGASSIZ, being called upon, remarked, that until recently few bones of this description had been found in this section, and the finest specimens were those met with in making the Rutland Railroad, Vermont. Those, like the present specimen, belonged to the drift.

Two varieties had been met with in the country—the *broad* and *narrow* toothed, and it would be a question whether they should be considered distinct species. But specimens are so few, that it is not right to decide positively. Moreover, there are differences belonging to growth and sex. The present specimen belonged to a young animal, because the surface was not worn by grinding.

The most northern limits of the elephant in this country appear to be Behring Straits, and this leads to the question of their passage from Asia.

Prof. HUBBARD here made some brief remarks, and said he would show a piece of skin of the real Siberian mammoth.

On the Nature and Origin of the Species of Terrestrial Mollusca in the Island of Jamaica. By Prof. C. B. Adams.

Notwithstanding the difficulty of exploration in tropical regions, the Island of Jamaica presents remarkable facilities for the investigation of subjects which are connected with the geographical distribution of species. In the great number of typical forms of the terrestrial shells, and in the restriction of most of them, severally, to very narrow limits, we find the facts even more numerous than those which are expanded on the continent of North America over the whole temperate zone.

1. *On the Nature of the Species.* The first conclusion is this: that in many groups the species are distinguishable by types only, and not by well defined limits. This proposition was illustrated by a figure, in which species were represented by circles, many of which were in contact, and whose areas were sprinkled irregularly with dots, representing varieties. One central dot represented the type of the species. Some larger dots represented types of a value intermediate between that of species and ordinary varieties. The amount of difference between the types was represented by their distances. On the boundaries of the species, we find varieties which closely resemble their neighbors in the adjacent species, while their affinities with the central types of both species are so nearly balanced, that it is not really a matter of much consequence on which side of them the imaginary boundary line of the species is drawn. An exact representation of these relations would require the three dimensions of space. On a plane surface we have only an approximation to the truth. With the boundary lines, we represent the species as described in books; without them, we see the species as they exist in nature.

If all the examples of this kind should be enumerated, very few species would remain isolated. Of such, some might be united by further discoveries; while others might remain isolated, since it is a part of the general plan of organic nature that the spaces between the groups shall be unequal, so that some species, some genera, some families, &c., shall stand quite alone.

The principle is not peculiar to the terrestrial mollusca of Jamaica. The Naiadæ and the Melanidæ of the United States, which have been so thoroughly studied by Mr. Lea, of Philadelphia, are exactly in the same case, but the facts are expanded over wider geographical limits. The same is true of the snails and fresh water shells of Europe, of many

groups of marine mollusca, of fishes, of birds, and even of animals. The special investigation of varieties is rapidly filling up the gaps which were once supposed to exist between species. Not dissimilar is the case of the human species, which graduate into each other in such a manner that the fact is often used as an argument for confounding all the races in one species. Yet it is admitted that the differences between the human races are much greater than between many distinct species of animals.

Our conclusion is briefly expressed in the proposition, that species are of the same nature as genera; that is, are to be founded on types, whether or not an impassable vacuum can be found between the types. The second inference on the nature of the species and higher groups, is this, that the natural types are not susceptible of being wholly comprehended in a few successive ranks, in each of which all the types shall be of exactly equal value; but that there is an indefinite series of types within types, which are inequidistant. If in one group, as that of *Helix sinuata* and the kindred species, it is practicable to establish several species of a given value, in another, as that of *Cylindrella Maugeri*, it is impossible to find species of the same value. We must either make numerous species of less value, or regard the group as one species of greater value. The same doctrine is illustrated also by the comparison of the genus Helix, in which the number of distinct subtypes is very great, with Succinea, in which subtypes are indistinguishable.

There is no mathematical or physical reason why the generic form of Succinea should not have been repeated with a great diversity of subtypes. Yet it is so slightly modified in the species, that Dr. Pfeiffer has grouped them geographically. On the contrary, Helix contains nearly one hundred types, of all values intermediate between a generic and a specific value.

In the same manner numerous other genera, families, and orders in all departments of organic nature, may be compared, and the same result obtained.

Since the subtypes of species are distributed with great regard to locality, it is obvious that much of the perplexity which results from the graduation of species into each other, is avoided by those travelers who take but a few specimens from distant localities and by those collectors who are satisfied with a single well-characterized specimen of each species. Such collections are valuable as exhibiting types; but they very imperfectly represent the relations of types: as a small group of human figures,

of which one should be an Apollo, another a Congo negro, with two or three other as well characterized specimens, of distinct races, would very inadequately illustrate the natural history of mankind. It is obvious, also, that a difference of opinion between any two naturalists on the question, whether a given species is a good species, does not necessarily indicate a want of discrimination in the observers. It rather indicates that the type in question is a little above or below the rank into which it is attempted to force it. What shall we say now of the logical notion of *infimæ species*, which would both hypothetically characterize a species by unity of origin, and require us to find an impassable gulf between those species which are most closely allied. Such a doctrine only shows how the world would have been constructed, if the philosophers had made it. We will venture to affirm that the facility of discovering such species will be inversely as the knowledge of the facts.

2. *Our second topic is the Origin of the Species.* The common notion of *infimæ species* settles the question of unity or plurality of origin by definition! The facts conduce to the inference, that the existing species were introduced by the creation of many individuals, which were modelled according to certain types, that were mostly but not wholly local, and which differed from each other unequally, as do the existing varieties. The proof of this proposition is found in the geographical distribution of the varieties. In the great majority of species, the varieties are so distributed, that the space which is occupied by one of them coincides with that of other two or more. Now, if the circumstances of locality had produced the local types by modifications of one original type of the species, then all the varieties which inhabit a locality should have been affected. The geographical coincidence of one variety with several local varieties is inconsistent with any other theory than that of an original constitutional peculiarity of character in each variety. This inference is confirmed by the occasional intermingling in one locality of varieties, which differ from each other as much as those which occupy distinct regions. The same general mode of distribution holds in the case of entire species. Some are very local, and others, more widely distributed, occupy the ground of several local species. We have then indistinct varieties, distinct varieties, doubtful species, good species, and groups of species, and all the intermediate types, distributed in the same manner. Now, the theory of unity of origin requires us to believe that all the types which are below the value of a species are the effects of locality; and although specific types of exactly equal value in all groups do not exist, yet that the types

which are exactly of a specific value were created in one centre in a single stock, but that those types which are more comprehensive than species had a plural origin of exactly as many stocks as they contain good species! and that species of doubtful value would not be doubtful, if we looked at nature through the doctrine of the logical infimæ species.

Notice of a Reversed Cyclostoma.

In the extensive genus Cyclostoma, only one reversed specimen, so far as we can learn, has hitherto been known to Conchologists. It belongs to the small European species *C. scalarinâm villa*, and is in the collection of Dr. L. Pfeiffer.

Among the multitude of shells of the Cyclostomidæ, which have been accumulating for ages in Jamaica, I was unable to find one reversed specimen. Recently, however, the Rev. F. R. Holland has sent me from Jamaica a reversed specimen of C. Tayanum. The shell has attained nearly its full size, being 0.7 inch long, but wants the reflected lip of maturity. This species is one of the most abundant in the island.

Prof. Agassiz remarked upon this paper, that the question of the origin and nature of species was one of the greatest importance, as it necessarily involved the question of the unity of the human race, for man zoologically speaking is an animal, and the same arguments which apply to animals in this respect apply equally well to him: it is because it involves this question that naturalists have been placed in an awkward position, and charged with unfairness, and venturing on domains which do not rightly belong to them. I will not here enter upon a discussion of this matter, but in behalf of those men will make one remark, which is, that all our labors are in the animal kingdom, and that here is our legitimate domain, and that by keeping here all will come out well in the end. See what has been done in behalf of Natural Theology by being the ministers and interpreters of the works of the Creator, and in what does He speak so clearly as in His works, and yet we are constantly disturbed by the calumniations of those whose short-sightedness and ill-judgment does not enable them to see that all our labors tend to the propagation of their most vital points and doctrines; and it is for this reason that we are persecuted most by those who ought to be our strongest friends.

SECOND DAY, AUGUST 20, 1850.

(*Continued.*)

SECTION OF PHYSICS, MATHEMATICS, AND CHEMISTRY.

The Section was organized by the appointment of Prof. PEIRCE as Chairman, and Prof. W. B. ROGERS and Prof. E. LOOMIS as Secretaries.

The following communications were presented :—

1. ON SOME PECULIAR PROPERTIES OF A COMPOUND OF LARD AND ROSIN. By Prof. D. OLMSTED, of Yale.

By way of introduction, Prof. OLMSTED remarked, that he proposed to offer a brief paper upon a very plain subject,—too plain, perhaps, to suit the taste of some; but since it is the great practical object of this Association to promote the useful arts, and advance the welfare o society, he hoped the plainness of the subject would be no objection to it. He then proceeded with his paper, as follows :—

I do not know that notice has been taken by chemical writers of certain peculiar properties possessed by a compound formed of hog's lard and the common rosin of the shops. An accident first led me to observe something remarkable in this compound, and I have since made a few experiments, with the view of further investigating the relation between these two substances. Wishing to fit the brass plate of an old air-pump, so as to make a close joint with the receiver, I had been accustomed to apply to the plate a disk of leather saturated with lard. With the hope of rendering it more completely impermeable to air, I added to the lard a small quantity of rosin, and melted them together. I expected the rosin would give greater hardness to the lard, and make it fill the pores of the leather more effectually; but I was surprised to find that the change produced by the rosin, was to impart to the lard a tendency to remain in the fluid state, so that in a winter's day the compound, when cold, remained in the state of a semi-fluid, at the temperature of a room moderately heated. I found, also, that this preparation, when applied to the leather of the air-pump, rendered it peculiarly soft, and, at the same time, very impermeable to air, so as to form a good joint with the receiver. But what more particularly arrested my attention was this, that having inadvertently left the leather on the brass plate of the pump for nearly a year, during which time the use of the apparatus was discontinued, I supposed, when I took the pump out again, that I should find the plate much corroded, as I had sometimes seen it before, when exposed for a much less time to the action of the oiled disk of leather;

but, on the contrary, the brass was entirely free from corrosion, and I have uniformly found the same to be the case since, however long the leather may have remained in contact with the plate.

This observation showed another and more important use of the same preparation for lubricating the pistons, which being likewise of brass, and moving in brass barrels, had before occasioned me much inconvenience by their liability to corrode by the action upon the brass, of the oil used for lubricating. Moreover, the tendency of the preparation to assume the fluid state, by the heat excited by the friction of the piston, rendered it a very convenient and effectual application for this purpose.

I have recently made a few experiments with the view of ascertaining the melting point of this compound, and the proportions of the ingredients which give the lowest melting point. The best proportions are, by weight, lard three parts, rosin one part. If the rosin be added in fine powder, and the mixture be well stirred, even without the application of heat, it softens, and so nearly approaches a fluid, as to run freely when taken up on the stirring-rod, at a temperature of 72 degrees. On melting the mixture, and setting it aside to cool, the following changes take place:—At 90° it remains transparent and limpid; at 87° a pellicle begins to form on the surface, and soon after the compound begins to grow slightly viscid; and as the temperature descends, it passes through different degrees of viscidity, like oils of different qualities, until, at 76°, it becomes a dense semi-fluid. It is an unexpected result that the addition of one part in four of rosin, whose melting point is near 300°, to lard, whose melting point is 97°, should render it more fluid, reducing its melting point to 90°, imparting to it the properties of a semi-fluid at a temperature as low as 76°, and even rendering the preparation of a softer consistency than lard itself at a temperature as low as 60°.

This compound of lard and rosin has, therefore, two somewhat remarkable properties.

1. It prevents in the lard, and probably in all the animal oils and fats, their tendency to generate an acid, and thus to undergo spontaneous decomposition. A much smaller proportion of rosin than one fourth gives to lard this property, destroying as it does the tendency of this substance to oxidation. Several important practical applications result from this property. Its use for lubricating surfaces of brass or copper has already been adverted to. It is equally applicable to surfaces of sheet-iron. I have found a very thin coating, applied with a brush, sufficient to preserve Russia iron stoves and grates from rusting during summer, even in damp situations. I usually add to it a small

portion of black lead; and this preparation, when applied with a brush in the thinnest possible form, will be found a complete protection to sheet-iron stoves and pipes. The same property renders the compound of lard and rosin, a valuable ingredient in the composition of shaving soap. The quality of this article is greatly improved, and its harsh effects upon a delicate skin prevented, by using a much greater proportion of oil than is commonly employed, so as to saturate the alkali; but such soap easily becomes rancid when wet with water, and suffered to remain damp, as it commonly is when in use. But if a certain proportion of this compound be added to common Windsor soap (say one half of its weight), the tendency to grow rancid is prevented. For the sake of experiment, a very soft and agreeable shaving compound, or "cream," may be made, by steaming in a close cup a cake of Windsor soap, so as to reduce it to a soft consistence, and then mixing intimately with it half its weight of our resinous preparation, adding a few drops of some essential oil.

2. The perfect solution into which rosin passes when heated with oil, suggested the possibility of improving, in this way, the quality of oils used for illumination, and, by its reducing the melting point of lard, of rendering that more suitable for burning in solar lamps. I, therefore, added powdered rosin to lard oil, in the proportion of eight ounces of rosin to one gallon of oil, and applied a moderate heat, sufficient to produce perfect solution. I then filled two solar lamps, equal in all respects, the one with lard oil, the other with the same holding the rosin in solution, and regulated the flame so as to be as nearly of the same size as possible. I finally measured, by the method of shadows, the comparative intensities of the light, which I found to be as 7 to 5 in favor of the prepared oil. This burned with a flame of peculiar richness, plainly exceeding in density that from the simple oil; but after two hours, the flame of the prepared oil began to decline slowly, and soon became inferior to the other, an effect which doubtless arose from the clogging of the wick. I had hoped, on account of the perfect solution which the rosin seemed to undergo, that the compound would burn freely without encountering this impediment; but in this respect I was disappointed, and can only say that, if some means can be devised for avoiding the tendency to clog the wick, the addition of a small portion of rosin to lamp oil, or to lard, will add essentially to its value for burning in solar lamps, by rendering it less liable to congeal, and by increasing its illuminating power.

Prof. A. D. Bache agreed with Prof. Olmsted as to the importance

of bringing such practical matters to the notice of the Association. He considered the lowering of the melting point in this case as coming under the general law, that the fusing point of compounds is lower than that of either ingredient. Thus lead, dissolved in tin, lowers the melting point, and bismuth added to the compound causes it to fall still more, although lead is less fusible than tin, and bismuth than the mixture of lead and tin.

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2. Notice of two American Meteoric Irons. By Professor B. Silliman, Junior.

The first of these irons was found by Mr. Jacob Watters, of Louisville, on Salt River, a tributary of the Ohio, about twenty miles below Louisville, Kentucky. The mass which Prof. S. exhibited weighed about eight pounds when it came into his hands. It had been heated in a forge by its original proprietor, to remove a portion, and, in this process, the original form is somewhat defaced. Nothing is known of the time of its fall. Iron of the same mass is said to be in the hands of some person whose name has not yet reached Prof. S.

The meteoric character of the mass is apparent from its peculiar crystalline structure—the hard external crust—the masses of magnetic pyrites which are scattered in large rounded nodules through it; but especially by its chemical constitution, as is seen in the following analysis, made under the direction of Prof. S., by Mr. W. H. Brewer, of the Analytical Laboratory, in Yale:—

Eight grammes were dissolved in HCl, NO_6H added, filtered from the insoluble part, the filtrate divided in six parts, four containing each one gramme in solution, the other two containing each two grammes. Iron and nickel were determined in each of the first four; the other two tested for Al, Mg, and Co. The nickel was separated from the iron by means of succinate of ammonia.

Iron - - - - - - -	90.23	90.51	91.07	91.14
Nickel - - - - - -	9.68	9.05	9.68	9.05
Magnesium and Sodium			trace	trace
Insoluble - - - - -	26	26	0.26	0.26
	100.17	99.82	101.01	100.45

The amount of insoluble matter obtained from the 8 gr. was 0.21 gr., which, after long burning, weighed 0.16 gr. The loss was assumed as carbon. The residue, fused with carb. soda, gave traces of silica and

iron, very distinct, and nickel (?). The original meteorite also contained traces of sulphur. The amount of insoluble matter varied in different trials.

From these analyses it is evident that this is one of the most remarkable meteoric irons yet examined, in the large amount of nickel which it contains, being nearly ten per centum. A very careful qualitative examination of the insoluble residue failed to detect appreciable quantities of those numerous various substances—*i. e.*, cobalt, chromium, copper, manganese, which are often found in meteoric irons, as was first shown by Berzelius, and confirmed by many others. The peculiar form of carbon, however, which Berzelius first noticed, is found in small quantity in this specimen, and the remainder of the insoluble matter appears to have been a compound of silicon, nickel, and iron.

The second mass of iron noticed by Prof. Silliman was found in the State of Pennsylvania, near the city of Pittsburgh. The attention of Prof. S. was called to this mass by Mr. George Weyman, a student in the Analytical Laboratory of Yale College, and through him all the details of its history have been obtained, which can now be hoped for. from Mr. John H. Bailey, of Pittsburgh. Prof. S. then read extracts from a letter from Mr. Bailey, dated June, 1850, from which it appears that this mass of meteoric iron was found in a field upon Miller's Run, in Alleghany county, Pa., near Pittsburgh. A farmer was ploughing in the field, where, seeing a snake, he seized a stone, as he supposed, to destroy the animal, but, finding it remarkably heavy, he was attracted, after completing his purpose, to examine the body which possessed such a remarkable weight. It was carried to Pittsburgh. where it was found to be very malleable, and unfortunately wrought into a bar, which has since been lost sight of. The mass was of an orvidal figure, almost six or seven inches in diameter, and weighed nearly 292 pounds. It is greatly to be regretted that only a very small portion of this large mass has been preserved. A qualitative examination of it has shown that it is rich in nickel, and possesses only a very inconsiderable portion insoluble in acids. Prof. Silliman stated that he would present a complete analysis of the mass when he had received the portion still remaining, which is now on its way from Mr. Bailey.

In answer to an inquiry from Prof. W. B. Rogers, Prof. S. stated that no chlorine was found either in the Salt River iron, or that from Columbia, in South Carolina.

Prof. R. then stated his objections to the evidence of chlorine existing in meteoric irons, &c.

To which Prof. S. replied, that the general fact of the presence of choride of iron in meteoric masses could not be regarded as settled; but he had no doubt of its truth in special instances. He cited the case of the Claiborne (Ala.) iron, first noticed by Dr. Jackson. This iron contains so much of metallic chloride as to retain its polish only for a few hours even when covered with a glass bell. The surface is bedewed with a green solution of chloride of iron and nickel, which soon completely oxydizes the whole surface, and covers it with a thick incrustation of the oxyde of iron. In the same manner, the larger Texas mass of 16,000 pounds, now in Yale College Cabinet, when, some time since, a deep section was made from one end of it showed for a few days a similar bedewing, which, on examination, proved to consist of iron and nickel. This exudation or deposit ceased after a few days, and the surface of the section now retains its polish. Prof. S. inferred that, in this instance, the presence of metallic chloride could be safely inferred only from a deep section of the mass.

Prof. W. B. Rogers, referring to the chlorine detected in many meteorites, spoke of the extraneous sources to which, in most cases, it may be ascribed. He said that a fragment resting upon, or becoming embedded in, the soil of our tertiary region, or that of the great carboniferous area of the West, could hardly fail to contract a trace of chloride of sodium, from its continued contact with these marine deposits, all of which contain more or less of the substance.

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3. On the proper Height of Lightning-Rods. By Professor E. Loomis.

The rule prescribed by the French Academy of Science, and copied into almost all the works on electricity, for determining the proper height of a lightning-rod, is, that a rod will protect a circle whose radius is twice the height of the rod. A case recently occurred in Tallmadge, Summit county, Ohio, which appears to demonstrate that this rule is unsafe. On the afternoon of July 27th, about six o'clock, there was a slight shower of rain, accompanied by a few flashes of lightning. One flash was remarkably vivid, and was succeeded almost instantly by a loud report. In an instant afterwards, a large pile of shavings, lying on the west side of a carriage shop, was found in full blaze. The

shavings had recently been carried out of the shop, and were quite dry; and as no fire had been used in that vicinity for several weeks, and no other mode is known in which the shavings could have been ignited, it is inferred to have been caused by the electric discharge. The carriage shop was furnished with a lightning-rod, and it was a matter of surprise that the fluid should have struck the ground so near to the rod. The top of the rod was fifty-nine feet high, above the shavings, and the shavings were 100 feet distant from a point vertically under the top of the rod. According to the rule above quoted, this rod should have afforded complete protection to a distance of 118 feet from its base: whereas, the shavings were struck at a distance of 100 feet, and that, too, where, being elevated only a few inches above the general level of the ground, they might be presumed to afford no peculiar attraction for the lightning. This rod appears to have been constructed in accordance with the usual rule. It is terminated by three points, which are gilded, and appear to be in tolerably good condition. About ten feet from the top is a break in the rod, and the two portions are looped together. From this point, the rod is continuous to the bottom, and enters the ground to the depth of about three feet, where the earth, at the time referred to, was quite moist. The rod is about five-eighths of an inch in diameter. This case demonstrates, to my mind, that it is unsafe to rely upon a rod to protect a circle whose radius is more than once and a half the height of the rod, at least upon the west side, being that from which thunder-showers generally come in this latitude.

Prof. A. D. Bache inquired if Prof. L. was an advocate for three-pointed lightning-rods. Prof. Loomis replied that he had not found any difference in the effect of three and one, but that a large number of points certainly diminished the discharging action of the rod.

Prof. Henry said, the question of balls and points had not been fully settled. If electricity acts inversely as the square of the distance, then, on the principle of central forces, the induction on a sphere at a distance from the cloud would be the same as if all the matter of the sphere were concentrated in its centre, and consequently the attraction of the ball or sphere on the electricity of the distant cloud would be the same as that of a point. When, however, the inducing body, or the discharge itself, came near the rod, it would be much more strongly deflected by the point than by the ball, because the former would be electrified by induction to a much greater degree of intensity, for the same amount of electricity which would be diffused over the surface of the ball would

be condensed in the point, and hence it would tend to rupture the air, and thus give a more easy passage to the discharge.

His attention had been directed to the action of a ball at a distance, by the fixture on the dome of the Capitol at Washington of a lantern, terminated by a ball. This apparatus had been erected at a great expense, for the purpose of lighting the public grounds. It consisted of a mast reaching to the height of ninety feet above the apex of the dome of the Capitol, terminated by a lantern about five feet in diameter and six or seven feet high. In this were jet gas burners, equal in illuminating power, according to the statement of the projector of the arrangement, to six thousand wax candles.

After the whole apparatus had been prepared, I was, said Prof. H., requested to give an opinion as to the effect which the lightning might have upon it. My answer was, that it would attract the lightning from the heavens, and thought the building might be protected by good conductors from the lantern to the earth, yet no protection which the present state of science could devise would be as safe as no exposure—the very idea of protection involved that of a less degree of danger. Though in the case of the ordinary lightning-rod the lightning is seldom or never attracted from the cloud by the conductor, yet in this case the great height of the mast, the height of the dome above the ground, and the elevated position of the building itself, gave a total elevation bearing a considerable ratio to the height of the cloud—add to this, the great amount of metallic surface, and, above all, the large gas burner, and we have an arrangement well calculated to elicit a discharge from the cloud, when under ordinary influences no effect of the kind would take place. It is well known to the Section that the best apparatus for collecting atmospheric electricity is a long pole, with a wire along it, and a lantern at the upper end. The fixture on the Capitol was indeed an exploring apparatus on a magnificent scale. The result was such as the Professor had anticipated. The first thunder-storm which passed over the city after the erection of the lantern, discharged itself upon it, put out the light, and when the whole was taken down, several perforations were found melted in the copper ball which surmounted the lantern.

In this case the induction from the cloud took place over the whole surface of the lantern, and the attraction was in proportion to the number of particles in the surface of the metal. The principal action was, however, due to the stream of heated air from the burning gas.

Prof. HORSFORD spoke of the remarkable splintering of pine trees struck by lightning.

Prof. LOOMIS referred this to the imperfect conducting power of the resinous wood of this tree.

Mr. REDFIELD spoke of the comparatively good conduction of the beech tree, and called attention to the fact, that the lightning marks in these trees are found near or at the base. The form of the leaves of the pine tree might, he thought, render it more liable to be struck by lightning.

Prof. HENRY stated, that during his residence in Princeton he had embraced every opportunity of studying the effects of lightning, but it so happened that in the course of the fifteen years he had remained in that village the lightning had fallen but twice within its limits, while in a field about three miles to the south it had struck almost every year. He had, however, examined a number of trees which had been struck, and in every instance had found that the greatest dynamic effect was produced on the body below the junction of the branches. This he attributed to the concentration of all the power which had passed along the several branches to the trunk.

Mr. REDFIELD called attention to those storms which are attended with frequently recurring flashes of lightning, and little or no loud thunder.

Prof. OLMSTED spoke of a storm which had recently visited New Haven. The lightning was vivid by its continuity rather than the intensity, and it was accompanied by a constant roar of thunder; it was rather puzzling to him to know why there was apparently so much lightning, and so little damage done. He thought that in such cases the clouds are very high, which favors ramification, and diminishes the sound.

Prof. HENRY stated that there was one indication of the elevation of a cloud which he had frequently observed, viz., the color of the discharge. It was well known that when electricity was transmitted through a partial vacuum of rarified air, the color exhibited was purple. He had frequently seen lightning of this color, and had always considered it at a great elevation. He had also noticed that in cases of colored lightning, the discharge was very long. In one case, by observing the beginning and ending of the thunder, he had estimated the flash at not less than eight miles in length, and in another the flash was at least nine miles.

It frequently happened that the flash is given off from the lower edge of a cloud, and passed horizontally, or nearly so, to a considerable distance. In one instance, he had observed a flash to start from the edge of a cloud situated over an extensive basin, and strike a house five miles off, which was situated on the edge of the basin.

Prof. A. D. BACHE mentioned that the tin cones used as signals in the coast survey were frequently ruptured, while the pole on which they were placed was not injured. He was of opinion that Prof. Olmsted and his pupils should have determined experimentally the height of the clouds in the case referred to, and thus have verified or refuted the hypothesis.

Prof. OLMSTED mentioned the case of several inverted tin pans placed in a straight line on a bench in the path of the electric discharge, and that they were perforated on opposite sides as if by a bullet.

Prof. HENRY thought the phenomenon was in accordance with known electrical action. If a number of conductors are placed in succession in the path of a discharge, the end of the first, to which the lightning is passing, will become highly negative, while the other end of the same conductor must be highly positive; also, the first end of the second conductor will be negative, and the other end positive, and so on. The lightning, therefore, will enter the metal with much greater intensity than that with which it will pass along the conductor; and hence a hole may be melted at the point of entrance; for the same reason another hole might be expected at the point of exit, and in this way the perforations of the pans might be explained. The electricity did not pass through the space from side to side of the pan, as a bullet would have done, but took the circuit around the inverted bottom of the vessel.

He stated that in all cases when an electrical discharge passes through a conductor, the point at which the fluid enters, and that at which it passes out, are both marked with evidence of more intense action.

When a disruptive discharge takes place through the air between two conductors, in many cases a part of the matter of each conductor is transferred to the other. Prof. H. said that he had received accounts from different sources, of a remarkable phenomenon connected with this action. In the case of a person killed many years ago by lightning, while standing near to the whitewashed wall of a room, the discharge took place between his body and the wall, and on the latter was depicted, in dark color, an image of his person. Other cases of the same kind had been observed.

Prof. W. B. ROGERS called attention to a principle which he thought would explain many of the seeming caprices in the action of lightning, and which has an important bearing on the question of protection by lightning-rods. He explained that as the discharge is due to the opposite induction in the cloud and subjacent soil, the direction in which the great electric stream will move must depend upon the combined action of all the prominent points upon the surface of the ground and cloud; in other words, this direction is that of the *resultant* of all these local electric forces. The electric stream will, therefore, move towards the earth, in a line irrespective in a great degree of small local prominences, and may thus strike a building at an unprotected point, when there are lightning-rods quite near, even on the same roof, or may strike directly into the ground while there are more prominent objects, such as a house or a tree, near at hand. These phenomena are known occasionally to occur. From this view of the subject, Prof. ROGERS concluded that the question of the proper height of lightning-rods does not admit of any satisfactory answer.

ON AMMONIA IN ATMOSPHERIC AIR. By Prof. E. N. HORSFORD.

Prof. HORSFORD presented a paper in continuation of the communication made by him at the meeting of the Association in Cambridge, 1849, in relation to the ammonia in the atmosphere.

The relative quantities of ammonia found in rain and snow-water had been determined in a number of instances. The following table exhibits the results of several determinations, and their dates.

Ammonia in Rain and Melted Snow in one Cubic Metre.

	In rain.	In snow.
1849, Dec. 22,	1.56 gr.	
" " 29,		2.63 gr.
1850, April 4,	0.24 gr.	
" " 4,		0.72 gr.
" March 18,		1.49 gr.
" " 22, 23,		0.96 gr.
" July 16,	1.29 gr.	

Prof. HORSFORD announced his purpose to continue his observations through the coming year, that the subject might be fully investigated.

Prof. Rogers suggested that the action of acid on the glass vessels employed, and the difference of time during which the vessels were exposed to this action, might have something to do with the differences expressed in the table.

Prof. Silliman, jr., inquired whether the ammonia possibly present in the acid and water used, had been regarded.

Prof. Hosford, replied to the first suggestion, that he had in several instances, after calculating the ammonia from the platinum salt as usual, ignited the same, and, after washing and re-ignition, calculated the ammonia again from the metallic platinum remaining, and the results obtained by the two methods did not differ materially. To the query of Prof. Silliman, Prof. Horsford replied, that the ammonia in the acid and water used had been determined, and a corresponding deduction made from the results of the several determinations given in the table.

On the Variations in the Fundamental Star α Virginis. By M. E. Schubert, of Cambridge. Read by Lieut. Chas. H. Davis.

I have already stated to the Section that the labor of preparing a new theory and tables for the planet Mars had been begun in my office, under the immediate direction of Prof. Peirce. As a part of this undertaking, the duty was assigned to Prof. Schubert, whose very valuable services it has been my good fortune to secure, of going through a thorough revision of the observations of Mars made at Greenwich, by Bradley, Bliss, and Maskelyne, for the purpose of correcting accidental errors. In the course of his work, he was struck with the difference of the clock-errors given by α Virginis, and those given by other fundamental stars. The effect of this discrepancy was to make the R. A. of α Virginis in the Tab. Reg. appear too great.

When this was reported to me, I authorized Mr. Schubert to enter upon the investigation it suggested, and the result has been a very important and interesting addition to our knowledge of stellar astronomy.

By means of rigorous comparisons, made at various intervals, and with different fundamental stars, from 1764 to 1802, Prof. Schubert has shown, not only that there is an error in the tabular R. A. of α Virginis, but also that there are variations in the proper motion of this star, such as indicate an orbitual motion.

It is not my intention to enter here into a detail of the careful and circumspect methods by which Prof. Schubert has rendered his conclu-

sions satisfactory. They would occupy too much space, and will be given to the world in another form: but I will annex to this paper a tabular statement of the means obtained by the comparisons, from the year 1766 to the year 1802.

It is interesting to know that in making his first comparisons in order to confirm his original supposition of an error in the Right Ascension of α Virginis, Mr. Schubert found it necessary to reject α Canis Majoris, which he at first had selected as one of his tests, on account of its apparent deviations; and this determination was formed without remembering the announcement of Bessel, of the supposed variation in that star's proper motion. But in the case of Sirius, Bessel discovered only an increase of proper motion, the duration of which, and therefore the *precise* period of revolution also, remain to be determined by future observations, though Bessel surmises a period of about fifty years.

It will appear, on the other hand, from the subjoined table, that the alternate decrease and increase of the differences is distinctly appreciable in the case of Spica, thus affording a striking example and confirmation of the views advanced by the illustrious Bessel, in his discussion of the "variableness of the proper motion of the fixed stars"—which I will cite in his own words:

"The star, therefore, which exhibits this variation is a member of a system, limited to a small space. The variation, like the motions of the system, returns periodically, and this *periodic* return is indispensable, as the means of reconciling it with the errors arising from the undetermined progressive accumulation of its effect. If the system be known as a double star, the observation of a variable motion in one of its components is not surprising, since it must necessarily exist, and must constantly return again in the period of the revolution of both components about their common centre of gravity. But, on the other hand, if this variableness be perceived in a star which appears single, it leads to the supposition that the star is the only visible one of the components of a small system—a supposition only to be disputed as inadmissible on the ground that the quality of shining is to be considered an essential quality of a mass. The fact of there being innumerable visible stars is no argument against the existence of innumerable invisible ones. It is not doubted that the celebrated star of Tycho Brahe in Cassiopeia exists, though invisible."—*Astron. Nach. No.* 514.

I have extracted from Bessel's concluding papers on this subject, (A. N. No. 516) a table of comparisons of Sirius with β, α Orionis, α Can. Min., and placed it below by the side of Mr. Schubert's means

derived from comparisons with the same stars, to show the comparative amount, and rate of change, of the differences in each case. And I have to add that Mr. Schubert has recomputed the means of the Greenwich observations from 1835 to 1847, and finds the evidence of the alternate progressive and retrogade motion of Spica confirmed by these later comparisons.

The discovery of Mr. Schubert in the new path opened by Bessel gives increased interest to the inquiry began by that distinguished astronomer, and to the hypothesis to which that inquiry conducted him. The proper motions of other fundamental stars, in which there are indications of change, remain to be investigated; the character of those in which the proper motion is constant, is to be established; and these preliminary steps, while they will constitute in themselves important accessions to our knowledge of stellar astronomy, cannot fail to lead forward to further unforeseen improvement.

In conclusion, I will remark, that it is my desire and intention to supply every facility and encouragement for the continuance of these investigations, in doing which I shall only be performing a duty incumbent upon my present office.

Bessel's differences of Right Ascension of Sirius from the Tab. Reg., as shown by a comparison of 149 observations of β and α Orionis, and α Can. Min., from 1773 to 1803.

From	1773	May	21	to	1777	Feb.	17	—0.079	10	observations.
"	1777	Feb.	19	"	1777	Mar.	20	—0.037	10	"
"	1777	Mar.	21	"	1781	May	29	—0.014	10	"
"	1782	Jan.	25	"	1782	Mar.	23	+0.043	10	"
"	1782	April	5	"	1782	June	25	+0.092	10	"
"	1782	June	26	"	1782	Oct.	24	+0.166	10	"
"	1782	Nov.	12	"	1783	May	1	+0.089	10	"
"	1783	May	4	"	1783	Dec.	3	+0.106	10	"
"	1784	May	3	"	1785	July	2	+0.115	10	"
"	1785	July	4	"	1787	May	5	+0.224	10	"
"	1787	May	9	"	1787	Aug.	22	+0.179	10	"
"	1787	Aug.	25	"	1793	Sept.	24	+0.296	10	"
"	1794	June	18	"	1795	Aug.	17	+0.383	10	"
"	1795	Oct.	30	"	1802	Sept.	17	+0.258	10	"
"	1803	Feb.	18	"	1803	Aug.	19	+0.194	9	"

Schubert's means of differences of Right Ascension of Spica, corresponding to the means of the times, from 181 comparisons with β and α Orionis, and α Canis Minoris, between 1766 and 1802.

NOTE.—The arithmetical means of the times are given in order to exhibit a remarkable peculiarity in the signs of the differences.

1766	—0.123			—	13	=number of comparisons.
		—0.140	1768	—		"
1770	0.156			+	9	"
		0.134	1771.5	+		"
1773	0.111			—	9	"
		0.131	1774	—		"
1775	0.150			+	7	"
		0.124	1776	+		"
1777.5	0.098			—	13	"
		0.109	1778	—		"
1778.5	0.120			+	9	"
		0.091	1779	+		"
1780	0.061			+	12	=number of comparisons.
		0.044	1781	+		"
1782.5	0.026			—	12	"
		0.040	1783	—		"
1783.5	0.054			+	12	"
		0.040	1784	+		"
1785	0.025			—	16	"
		0.060	1786	—		"
1787	0.095			—	10	"
		0.111	1788	—		"
1789	0.127			+	9	"
		0.105	1790	+		"
1791	0.082			—	14	"
		0.124	1793	—		"
1795	0.165			+	12	"
		0.128	1796	+		"
1797	0.091			—	9	"
		0.107	1798.5	—		"
1800	0.122			—	7	"
		—0.126	1801	—		"
1802	—0.130				8	"
					181	=sum of comparisons.

Prof. MITCHELL inquired respecting the distance and magnitude of this invisible body.

Prof. OLMSTED made some remarks upon the contributions of the late E. P. Mason to astronomical science.

Prof. LOOMIS remarked upon the magnitude of the image of Sirius and other bright stars.

Prof. HUBBARD alluded to the irregularities noticed in the observations of α Lyræ and some other bright stars.

Mr. G. P. BOND considered it desirable that observations of the brighter stars should be continued, because those stars had been long observed.

Dr. GOULD remarked upon the advantage of employing stars below the first magnitude for the determination of clock errors.

THIRD DAY, WEDNESDAY, AUGUST 21, 1850.

GENERAL MEETING.

(*Morning Session.*)

The Association met in General Session in the College Chapel, at 9 A. M.

A report was presented from the Standing Committee, recommending that the Section of Physics, Mathematics, and Chemistry, be divided into a Section of Physics and Mathematics, and a Section of Chemistry and Mineralogy. The proposed alteration was accordingly authorized by the Association.

The President announced that the Standing Committee had unanimously recommended Prof. L. Agassiz for President of the Association for the ensuing year. Whereupon, said recommendation was unanimously adopted.

The election of new members having been gone through with, it was voted, in pursuance of the recommendation of the Standing Committee, that the afternoon sessions of the General Meetings be restricted to the reading of communications, without discussion.

A note from Mr. William Hillhouse, inviting the members of the Association to his house on Friday evening, August 23, was received, and the invitation accepted.

A letter was received from Mr. James Brewster, inviting the members to visit the collection of Historical Paintings in his Hall in Chapel street.

After a brief explanatory statement by Prof. Silliman, the thanks of the Association were voted to Mr. Brewster, and the invitation accepted for the hour between 8 and 9 A. M., of August 22.

It was then determined that the afternoon session of the General Meeting shall commence at $2\frac{1}{2}$ P. M.

The President announced the appointment of Prof. John Leconte, of Georgia, as one of the Assistant Secretaries of the Association, in place of Dr. B. A. Gould, jr., excused. He also reminded the members of the importance of entering their names on the register, and of furnishing to the Secretary abstracts of their papers and remarks.

The reports of Committees were then called for.

1. *Committee on the Communication of Lieut. Maury, upon Winds and Currents.*

The PRESIDENT stated that the Report had been presented at the Charleston Meeting in March last, and published in its proceedings.

Prof. J. H. COFFIN remarked, by way of explanation, that his own contributions to that Report had failed to reach the Chairman in time.

2. *Committee on the Prime Meridian.*

Lieut. DAVIS presented a Report, which, after being read, was referred to the Standing Committee.

3. *Committee on the subject of an uniform Standard of Weights and Measures.*

Prof. HENRY reported that the matter was still in progress, and that a circular letter on the subject had been prepared and would be distributed among the scientific men of the country.

Dr. HARE desired to call the attention of the Committee to the importance of providing means for subdividing the grain into smaller weights, by weighing, instead of being wholly dependent upon mechanical division.

4. *Committee on the Publication of an Astronomical Journal.*

Dr. B. A. GOULD stated that he had prepared a paper on this subject, which had been assigned for reading at a meeting of the Physical Section, so that a report here would be unnecessary.

The Committee was then discharged from any further consideration of the subject.

5. *Committee to memorialize the State Governments on Geological Surveys.*

Prof. H. D. ROGERS reported that the Committee had discharged the duty assigned. Whereupon, the Committee was discharged.

6. *Committee to memorialize the Legislature of Pennsylvania in reference to the publication of the final Geological Report of that State.*

Mr. REDFIELD reported that, owing to various causes, the Committee had not yet finished the business with which it was charged. President Hitchcock, Chairman of the Committee, being absent from the country, it was voted that the Committee be completed by the election of a member, to be nominated by the Standing Committee.

7. *Committee in Relation to the Coast Survey.*

Prof. PEIRCE stated that the Committee had already made a partial report, and desired that their commission should be continued, and that

two members should be added to their number, to be nominated by the Standing Committee. This request was agreed to.

8. *Report of the Treasurer.*

The TREASURER presented a verbal report of the receipts and expenditures of the past year, and stated that many of the members had failed to pay their annual assessments.

It was then voted to refer the Treasurer's accounts to a Committee of two Auditors, to be appointed by the Chair.

Prof. B. SILLIMAN, jr., and Prof. E. N. HORSFORD, were appointed said Committee.

THIRD DAY, AUGUST 21, 1850.

GENERAL MEETING.

(*Afternoon Session.*)

The following resolution, reported by the Standing Committee, was unanimously adopted by the Association:—

Resolved, That the President be requested to return the thanks of the Association to the Mayor and Corporation of the city of Charleston, for their signal liberality in the publication of the Proceedings of the Meeting held at Charleston, and for the donation of three hundred copies of the volume, by which the funds of the Association have been materially aided.

The following papers were then read:—

1. ON THE LATE PERIODICAL VISITATION OF THE AURORA BOREALIS. By Prof. D. OLMSTED.

(*Abstract.*)

The leading object of this paper is as follows:—

1. To establish the *fact* that we have just passed through an extraordinary period of auroras, (to which I venture to give the name of *Visitation,*) which commenced in 1827, and closed in 1848.

2. To determine the *characteristics* by which these exhibitions of the aurora differ from the ordinary exhibitions of the same phenomenon,

and to place on record a full and accurate description of several of the most remarkable, as materials for comparison in future visitations.

3. To establish, by an extensive collation of observations, the *laws* of the aurora borealis.

4. To determine the *origin* of the aurora, and to assign the true *cause* of the phenomena.

In order to establish the *reality* of the occurrence of an extraordinary visitation of the Aurora, within the period assigned, we first examine all the facts of the case, comprising a complete history of the phenomenon for the period in question; and, secondly, compare the exhibitions of this period with the ordinary displays of the Aurora for the same length of time, attempting to prove that, during the period called a "visitation," the auroras greatly exceed the ordinary exhibitions of the meteor, in number, in splendor, in peculiar combinations of form, and in a progression which marks a beginning, middle, and end.

The laws of the aurora borealis, as determined by an extensive induction of facts, chiefly gathered from personal observations, we endeavor to show to be the following,—comprehending both such as are generally admitted to be the leading facts, or laws, and such as are deemed somewhat new, or at least not universally received as established truths.

1. *Time of Occurrence.* An aurora of the first class usually occurs near the end of evening twilight, in the form of a northern light, resembling the dawn; commonly arrives at its maximum between 10 and 11 o'clock, and frequently a little before 11; and auroras of the highest order frequently continue all night, while those of an ordinary character commonly end before midnight.

2. *Material of the Aurora.* A great aurora is usually preceded by a large bank, or cloud, of a peculiar vapor, differing in its nature from ordinary clouds, commonly exhibiting a milky appearance, but sometimes a smoky hue, or the two mixed together; and the extent and density of this auroral vapor, resting upon the northern horizon, forms the best prognostic we have of the probable intensity of the exhibition which is to follow, composing, as we believe, the material of which the successive forms of the aurora are constituted.

3. *Auroral Waves.* The auroral waves, when peculiarly grand, make their appearance later then the streamers and arches, and usually later than the corona, continued to a later hour of the night, appear at a lower level than the streamers, and roll upwards, in the direction of the streamers, toward the point of general concourse.

4. *Extent.* Auroral exhibitions of the higher order are commonly of great extent, spreading over no inconsiderable part of the earth's surface, and reaching to a great but variable height.

5. *Periodicity.* Auroras of the first class have three distinct forms of periodicity,—a diurnal periodicity, commencing, arriving at their maximum, and ending at definite hours of the night, as already asserted; an annual periodicity, rarely or never occurring in June, and the greatest number of the higher order clustering about November, these last bearing a striking resemblance to each other; and a secular periodicity, the most remarkable of all, recurring in great series, which we have denominated "auroral visitations." The visitations most marked and best defined, occur at intervals of about sixty-five years, reckoning from the middle of one period to the middle of the next period, and last from twenty to twenty-two years, making the interval from the end of one to the beginning of the next about forty-five years.

6. *Relations to Magnetism.* While the forms of the aurora usually appear to be under the control of magnetic forces, yet this is not always the case, since the arches do not always culminate in the magnetic meridian, nor do they always place themselves at right angles to that meridian, nor does the effect on the needle correspond to the different states of intensity of the aurora.

7. *Geographical Relations.* The aurora borealis has remarkable geographical relations, belonging chiefly to the higher latitudes, and only in the great visitations descending below the latitude of 40°, but descending lower on the western than on the eastern continent, and prevailing more in the northern than in the southern hemisphere.

The foregoing seven propositions appear to embrace the most important facts respecting auroral exhibitions, and are, therefore, deemed to be, in the present state of our knowledge, the laws of the aurora borealis. But we cannot rest satisfied to let this great visitation of the phenomenon,—which, in its whole progress and duration, we have enjoyed fair opportunities of observing,—pass by, without an effort to assign its true origin, and to explain the causes of its mysterious phenomena.

After thunder and lightning were first proved by Franklin to be caused by electricity, it was taken for granted, almost without discussion, that the aurora borealis is produced by the same agent; and this hypothesis has still very numerous adherents. We endeavor then, first, to show that the aurora borealis is not produced by *electricity*. We argue—1. That it is unsafe to infer an identity of origin from a resemblance between the aurora and certain appearances of electricity in passing

through an exhausted tube, and that the resemblance itself is greatly overrated. 2. That such an origin is inconsistent with the great extent of the phenomenon. 3. That electrometers do not indicate the presence of electricity in any unusual degree, during an aurora. 4. That these exhibitions are scarcely known in the equatorial regions, where electricity is most abundant, and prevails most in the polar regions, where thunder and lightning are unknown. 5. That this cause is incompetent to account for the auroral vapor, the material of the aurora itself. 6. That the motions of the auroral vapor, in its corruscations, are too rapid to be caused by electrical attractions and repulsions, but not sufficiently rapid for the electric fluid itself. Finally, that electricity is inadequate to account for the periodicity of the aurora, if not entirely inconsistent with the secular periodicity.

But *magnetism* has more claims than electricity to be considered as the true cause of the aurora borealis, since it is acknowledged that the forms and positions which the streamers, the arches, and the corona assume, are intimately related to magnetism, and that the magnetic needle itself confirms and establishes this relation. But this proves merely that the matter of the aurora has magnetic *properties*, but decides nothing with respect to the *origin* of the aurora, which is the principal thing to be accounted for; while magnetism, like electricity, is inadequate to account for the extent, for the light, for the motions, for the material, and especially for the periodicity of the aurora.

Dissatisfied with the attempts which have been made to account for the origin, or to explain the phenomena of the aurora, from either electricity or magnetism, or from any other cause of a terrestrial nature, we next look for the source to the planetary spaces, and arrive at the conclusion that the origin is *cosmical.*

The *phenomena* of the aurora belong, indeed, to the atmosphere, since they are affected by the earth's diurnal rotation; but the *source* of the matter of which the meteor is composed, we hold to be derived from regions above the atmosphere. This distinction we deem to be of much importance in investigating a true theory of the aurora, viz., that the *exhibitions* are atmospheric, while the *source* or *origin* is cosmical. We, therefore, inquire, first, at what *height* in the atmosphere the usual exhibitions take place? We conclude that while some of the forms of the aurora, as the corona, are unsuitable for this investigation, being merely the effect of perspective, yet that there are other forms which may be used for parallax no less than a common cloud, as the upper line of a well defined arch, or some single fragment of an auroral cloud, uncon-

nected with other parts of the exhibition, and of a form so peculiar as to arrest the attention of observers at some distance from each other. From an extensive comparison of data, we arrive at the conclusion that the height of the aurora is very variable, sometimes reaching an elevation of at least 160 miles, and at other times rising but little above the region of the clouds; still we attempt to prove that the testimony of Farquharson, that the aurora is sometimes seen below the clouds, is not conclusive; nor that of Parry, Richardson, and others, who have asserted that, in the Polar seas, auroras sometimes appear between the spectator and icebergs, at a little distance.

It is required of a *theory* that it be a legitimate deduction of well-established truths; and it is required of a hypothesis, that it explain the leading facts, and that it be not *inconsistent* with any known facts, although its *application* in certain cases may not be readily perceived. An explanation which unites the characters of both, which is at once an inference from acknowledged truths, and which affords an adequate solution of the leading phenomena—such an explanation is deemed peculiarly worthy of confidence until a better can be proposed. In conformity with these principles, we endeavor, first, to infer the cosmical origin of the aurora borealis from known facts; secondly, we investigate, as far as we can, the nature of the body, or bodies, from which the material of the aurora is derived; and, thirdly, we apply the theory thus formed to the general facts or laws of the phenomena. In the first place, we argue the cosmical origin of the aurora, from the *extent* of the exhibitions, which is greater than could arise from any terrestrial emanations, or atmospheric precipitations; from the *velocity* of the motions, which is too great for any terrestrial forces; from the occurrence of the different stages of an aurora, (the beginning, maximum, and end,) at the *same hour of the night* in places differing widely in longitude,—a fact which indicates that successive portions of the earth's surface, in the diurnal rotation, come under the origin of the aurora situated in space; finally, from the *periodicity* of the exhibitions,—the diurnal, which shows a relation to the position of the sun with respect to the horizon,—the annual, which indicates a relation of the auroral body to the earth's orbit,—and especially the secular, implying a cycle, at the end of which the auroral body and the earth return to the same relative position in the heavens, while the very existence of such a periodicity takes the phenomena out of the pale of terrestrial, and places it within the pale of astronomical, causes. In the second place, we infer that the auroral body, whence the material of the aurora is derived, is a nebulous body,

consisting of exceedingly light, inflammable, semitransparent, magnetic matter; that it has a revolution around the sun; that probably there are many such collections of nebulous matter diffused through the planetary spaces, each having a revolution and period of its own; and that the phenomena of the aurora arise from one of these coming occasionally into the earth's sphere of attraction. In conclusion, we endeavor to show that the cause here assigned and proved to exist is adequate to account for the leading facts of the case, as the great extent and velocity, the geographical position, the magnetic relations, and the periodicity; while, in regard to the minor facts, it is inconsistent with none, although it may not be ready to explain them all until all the modifying circumstances are fully understood.

It may be remarked that there is, in my view, a great analogy in the origin of the aurora borealis and of meteoric showers. I hope hereafter to be able to develope this analogy more fully than can be done on the present occasion, and to investigate the question of any possible connection of either phenomenon and the zodiacal light.

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2. On the Nautical Almanac. By Lieut. C. H. Davis.

At the meeting of the Association at Cambridge, it was made known that Congress had evinced a determination to establish a Nautical Almanac, by appropriating a small sum for its preparation; and I presume it will be expected of me to say something to the Section of what has already been done, and what it is expected to accomplish hereafter.

I should not venture to speak upon this subject before any other assembly than this of mathematicians and astronomers, who will understand how much, or rather how little, could be done in the short time and with the limited means hitherto employed.

And I fear that my brief communication will appear to consist rather of promises for the future, than of the recital of past performances.

I may be assured of receiving your approbation of the desire I felt, when called upon to superintend the Nautical Almanac, that the work should be creditable as well as useful to the country; that while it served the present purposes, it should endeavor also to improve the present means, of safe navigation; that his own labors should be turned to the best account, in order to render the predictions the most valuable to the practical astronomer; that it should make worthy contributions

to astronomical science, the most honorable form of acknowledgment of our obligations to those upon whom we have until now depended; that it should add its own testimony to that of other publications to prove that this noble science was not neglected among us; and that it should promote the pursuit of that science by encouraging, and I shall not be misunderstood when I say, in some measure compensating, some of those laborers in this field, who, though eminently worthy of their hire, have as yet borne the heat and burthen of the day without any other reward than our gratitude for the honor they have conferred upon the country, and that good name which is better than riches.

I considered that it belonged to my office to advance that which is, and always has been, the principal object of astronomy; and that is, in the language of Bessel, to supply precepts by which the movements of the heavenly bodies, as they appear to us from the earth, can be calculated. All else that we can learn of the heavenly bodies,—for example, their appearance, or the nature of their surfaces,—is not, indeed, unworthy of attention, but does not embrace the purely astronomical interest. Whether the mountains of the moon are formed in one way or another, is of no more concern to the astronomer than is a knowledge of the mountains of the earth to one who is not an astronomer. Whether the surface of Jupiter exhibits dark bands, or appears equally illuminated throughout, engages as little his curiosity; and even the four moons themselves interest him only by their motions. The problem that has been, and is to be, solved by astronomy, is to acquire such a knowledge of the motions of the heavenly bodies as to be able to give a satisfactory account of them at all times.

Something should be done towards the solution of this problem in the office of the Nautical Almanac. The example had been set us abroad of making an Astronomical Ephemeris something more than a book of mere results of calculations based upon rules furnished elsewhere; it should itself help to investigate the theories it is obliged to employ.

And I have taken the most effectual means of carrying out these views, and of securing the highest character to the Almanac, by engaging the co-operation and advice of our eminent associate, Professor Peirce, of Cambridge, whose name affords a sufficient guaranty to the Association, to the country, and to the whole scientific world, that whatever is undertaken in theory will be creditably executed.

I also invited several other gentlemen of distinction to assist in this

work of theoretical preparation; not, however, with as much success as I could have wished.

The foundation of the Nautical Almanac was laid at a fortunate moment. It was recently announced, in the Royal Astronomical Society's notices of April 13, 1849, that the solar tables, begun by Bessel, and continued by Hansen, were nearly or quite ready for the press. Some time will probably elapse before the lunar tables of this astronomer, (for the perfection and completion of which the European astronomers, generally, have united to afford him every facility,) will be given to the scientific world. I expected to receive the solar tables by the time the question of the meridian—for which the Almanac is to be computed—was decided; but believing, from the best information I could obtain from Dr. Gould, that we should have to wait two years or more for the lunar tables, I adopted the recommendation of Professor Peirce, and commenced the preparation of tables for provisional use, by enlarging the early edition of the tables of Damoiseau, with Airy's corrections united to Plana's theory, with the corrections also due to the recently discovered inequalities of Hansen, and with those derived by Mr. Airy from the Greenwich observations. A new set of tables of the planet Mercury has been commenced, and has advanced considerably.

They are based upon Le Verrier's theory, published in the Additions to the *Connaissance des Temps* of the year 1848.

They are of a new form, adopted with a special regard to the convenience of calculation, being computed for three rectangular co-ordinates with reference to a plane passing through the sun, parallel to the earth's equator. Le Verrier's admirable improvement, by which the multiplied arguments of the old form of the tables are rejected, and the time is taken as the only argument for obtaining the three heliocentric co-ordinates, has been introduced, and will be retained hereafter in all new tables that may be prepared for the use of the Almanac.

The first step, and an arduous one, in preparing a new theory and tables of Mars, has been taken, by a thorough revision of the Greenwich reductions of that planet, from 1751 to 1802. Prof. Schubert, to whom this duty has been assigned, has given the most conclusive proof of the intelligence and accuracy with which he has executed it, by an important discovery, of which I shall make a separate communication to the Section, as well as by the correction of the erroneous value of the equatorial intervals of the wires of transit instrument between 1762 and 1765, which have been communicated to the world in Dr. Gould's astronomical journal.

Professor Peirce's recent development of the general formula of the perturbative functions of planetary motion, published also in the same journal, was made for the use of the Almanac office, and he has since added a new method of computing the constants of the function, and the differential co-efficients, by means of certain auxiliary series which approximate as much more readily than Le Verrier's series, as Le Verrier's, than the original series of Le Place and Legendre. Mr. Runkle, an assistant in the office, has computed the values of the co-efficients of Peirce's fundamental auxiliary series.

Before, however, this new method of Prof. Peirce was invented, I had invited Mr. Sears C. Walker, who was assisted by Mr. Pourtalès, to compute the values of the co-efficients of M. Le Verrier's series for the development of the disturbing force. In the course of his labors he has been led to the invention of an ingenious and comprehensive symbol with variable indices, and by the use of this symbol to a greater simplification of Le Verrier's transformations of the original formulæ of Le Place for the computation of the co-efficients. I have requested Mr. Walker to communicate this new form of notation, and the mode of using it, to the Section.

It is not, of course, unknown to the Section that I have communicated Mr. Walker's Ephemeris of Neptune for the present year to the Smithsonian Contributions, and also the occultations of the planets and fixed stars by Mr. Downes of Philadelphia.

One of the assistants of the Nautical Almanac is employed upon the final computations of the perturbations of Uranus by Jupiter and Saturn, according to the formulæ of the inequalities given by M. Le Verrier at the conclusion of his paper upon the perturbations of the elliptic motion of Uranus, in the *Connaissance des Temps* for 1849.

Professor Peirce's corrections will also be applied to the elliptic elements of this planet; the perturbations due to the action of Neptune have already, as is known to the Section, been computed by him. I am happy to have it in my power to say that this gentleman has undertaken to revise the whole theory of Uranus: his revised theory will be made the basis of a new set of numerical tables.

The Section will be glad to hear also that I have secured the services of several gentlemen in the practical computations, whose ability is well known to the mathematicians and astronomers of the country; and I cannot deny to the Section and to myself the pleasure of especially mentioning, among my associates, my distinguished and accomplished

friend, Miss Maria Mitchell, of Nantucket, whose accuracy, fidelity, and learning, render her a most valuable assistant.

At this early stage of our existence, I am not prepared to communicate to the Section any detailed information as to the plan of the work, or the methods of conducting the practical computations.

These details are reserved for a future meeting of the Association.

I shall have, however, occasion to invite attention to the solar eclipse of July 28, 1851, to the means taken to facilitate and encourage its observation, and to the advantages we expect to derive from a comparison of our predictions with observation.

I very gladly avail myself of this opportunity to make known the obligations which the work in my charge, and the cause of science therefore as far as it has been promoted by its establishment, is under to the Honorable William Ballard Preston, late Secretary of the Navy, whose liberal sentiments and enlightened views encouraged the foundation of the Almanac upon the broadest basis of usefulness and honor. And when I consider how many obstructions might have arisen from a want of sympathy with the highest objects of science, or a want of regard for the means by which those objects are advanced, in the Head of the Department having charge of the work, I cannot refrain from expressing also my sense of personal indebtedness to this gentleman.

Since he has left the office in which he presided with so much firmness, judgment, and courtesy, I am enabled by his return to private life to give a full and cordial utterance to my feelings and opinions.

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3. Suggestions on Changes of Level in North America, during the Drift Period. By Prof. C. B. Adams.

Prof. Adams said: It is not the design of this communication to discuss the glacial history of the drift; its object requires that this theory should be assumed as a fact, and used as a point of departure.

I. The objections to the glacial theory lie against the origin of the required glacial sheet, rather than against the dynamics of the theory; while the objections to the other theories lie against their dynamics with a serious if not with a fatal force. The suggestion, therefore, of a possible cause of a vast glacial sheet, 5,000 feet thick, may not be useless, even if somewhat improbable. The hypothesis is this, that the glacial sheet was produced by a great elevation of land in the northern

regions above the present level. Besides the direct refrigerating influence of elevation, the flow of the tropical waters into the North Atlantic may have been essentially diminished by the contraction of the area of the ocean. This contraction must have been much greater, if, as is highly probable, the similar elevation of Northern Europe was synchronous with that of North America. Whether or not this elevation can account for the origin of the glacial sheet, there is direct evidence of a greater elevation than the present during the drift period, in the continuation of the drift striæ beneath the sea level, for it is well known that glaciers cannot advance into the sea. Unfortunately, the impossibility of following the striæ excludes us from the knowledge of the most important fact—the greatest depth at which striæ exist. The passage of drift materials across basins which are now filled with water, suggests the same conclusion. Such of the light materials of Cape Cod, Nantucket, and Long Island, as could not have been taken from the mainland by marine agencies, must have been carried by glacial agency, and consequently the intervening basins must have been above the sea level. The passage of the glacial sheet across the basin of Lake Superior presents some difficulties, which are diminished by the theory of elevation. The bottom of this basin is now at least one hundred and sixty-five feet beneath the sea level. Without elevation, drainage would have been impossible.

II. It is easy to ascertain, approximately, the amount of *submergence* which closed the glacial period. Submergence to a depth of not less than two thousand feet below the present level, effected the dissolution of the glaciers, and introduced the pleistocene period.

1. The general proof of such submergence is the present existence of altered drift at a corresponding elevation.

2. Special proof is found in the existence of osars at great elevations. A semicircular one, well characterized, occurs in the town of Peru, Vermont, at an elevation between one and two thousand feet.

3. Other special proof may be found in the lines of ancient sea margins, which are much more rare than the fluviatile terraces of a later date. Such a margin was found by President Hitchcock, in Pelham, Massachusetts, at an elevation of one thousand two hundred feet.

4. Perhaps the most convincing evidence is to be found in the existence of regular stratified deposits of clay and fine sand at the summit level of longitudinal valleys. An interesting example occurs in the town of Glover, in the northern part of Vermont. Until June 6, 1810, the summit level of a north and south valley, which extends through this

township, was occupied at a height of 1,500 feet above the sea level by a pond one mile and a half long and half a mile wide, which discharged its waters southerly into the Lamoille river. On that day a small channel was cut through the north barrier by some persons, with a view to the benefit of the mills in the north part of the valley. This barrier consisted of fine loose sand, with a thin coat of clay. A large channel was instantly cut by the water, and the pond was emptied in fifteen minutes. It then appeared that the basin was in a deposit of very regular horizontally stratified sand. In composition and structure, the deposit is exactly similar to those which occur in all the mountain valleys in that region, and which were at first supposed to be of fluviatile origin, because they commonly border the streams. A fluviatile origin, however, is not in any case a necessary inference from their position along the valleys. At the summit level, such an origin is impossible. Neither could the origin have been in lacustrine agency, for this would involve the absurdity of supposing that the pond formed its own barriers. It was obvious, also, that no unequal elevation of New England has changed the site of the summit level, since, in this and other similar valleys, the deposits extend through the entire length of the valleys. We are, therefore, compelled to admit the theory of a submergence sufficient to have formed deposits of 100 to 200 feet in thickness, at a level which is now 1,500 feet above the sea.

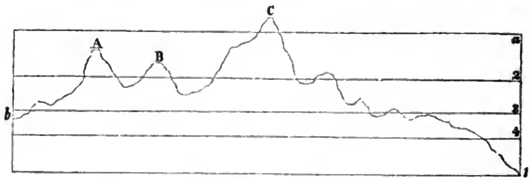
An obvious objection occurs in the want of marine fossils, which in Vermont are restricted to the valley of Lake Champlain, and to places not exceeding 250 feet above the present level. Diligent efforts have been made, in vain, to remove this objection in the manner most satisfactory—the actual discovery of such fossils in the higher masses of altered drift. It should, however, be remembered, first, that the introduction and increase of testaceous mollusca in the seas, which took the place of the glacial sheet, is likely to have been a slow process, so that possibly emergence may have commenced before their universal distribution; or, secondly, that in consequence of the extreme porosity of most of these deposits, the shells may have been entirely dissolved by the percolation of carbonated water, in those deposits which have been longest emerged; and, thirdly, that the deposits which abound in marine shells are locally continuous, in the valley of Lake Champlain, with a much greater extent of similar deposits at a somewhat greater elevation, which are totally deficient in fossils.

The time when the submergence commenced is indicated, not only hypothetically, because submergence was probably the cause of the dis-

solution of the glaciers, but demonstratively, by the following fact. The polished and striated limestones and marbles of the valley of Lake Champlain are not covered with drift, but to a great extent with a pleistocene fossiliferous deposit of clay, that has perfectly preserved the polish and fine striæ, which a very short exposure to atmospheric agencies obliterates. It is evident, therefore, that the submergence immediately followed the final dissolution of the glaciers.

III. The last process of emergence has been interrupted by at least one long period, during which the land was stationary at a depth of 400 to 500 feet below its present elevation. The fact of such a stationary period is so well known, that I will only allude to a beautiful illustration in the south-east part of the valley of Lake Champlain. In this region there are many hundred hills, with elevation varying from 800 feet downward. These hills are covered with drift. Up to a very uniform level of 400 feet, the drift is covered with pleistocene clay, which thins out in ascending to this level, where it invariably terminates. This fact is so invariable, that it may be used as a measure of elevation. So well defined, too, is the clay margin, that islands not exceeding an acre in extent are almost as obvious as if now surrounded by the pleistocene waters. Although remarkable, this fact of exactly defined sea margins need not appear incredible, since they occur only on the inner pleistocene islands, next to the Green Mountains, but not on the outer ones of the group, which were exposed to the waves brought by the north-west gales from the Canadian Sea into the Champlain Sound. On those outer islands, for a considerable distance below the level of 100 feet, there is an intermixture of materials derived from the drift with the exterior coat of clay, and consequently *some* of the pebbles and boulders appear on the surface of the clay. This class of facts has given rise to a hypothesis of a second drift period. It is very obvious, after a careful examination of these local facts, that they are the results of the agency of the waves of the pleistocene sea.

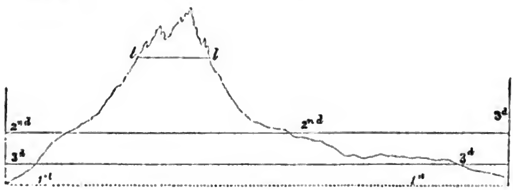
The accompanying figure exhibits the relations of land and water during this period in the New England States.



IDEAL SECTION OF NEW ENGLAND.

A B. Green Mountains.
C. Mt. Washington.
a. Upper limit of the drift agency.
b. Lake Champlain.
1. Sea level during the drift period.
2. " " at the beginning of the pleistocene period.
3. " " stationary during a part of the pleistocene period.
4. " " at the present time.

A similar series of events in Scandinavia was described by M. Desor, in the Bulletin de la Société Géologique de France, of Nov. 16, 1846. The following copy of M. Desor's section will suggest the obvious points of comparison.



l l. Limit of glaciers.
1*st* 1*st*. Sea level at the commencement of the drift period.
2*d* 2*d*. Sea level at the commencement of the pleistocene period.
3*d* 3*d*. Present sea level.

The marine scenery of this part of North America during this stationary part of the pleistocene period must have exceeded in beauty anything which now remains. The Champlain Sound with its thousand islands, the crowded archipelagoes and peninsulas of Maine, Massachusetts, Connecticut, and New York, enclosing tranquil straits and bays, must have presented, on a grand scale, those beauties which now appear in miniature in Boston Harbor and Penobscot Bay.

4. On the Coal Formation of the United States, and especially as developed in Pennsylvania. By Prof. Henry D. Rogers.

The principal topics discussed in this paper, the whole of which, for want of time, was not submitted to the meeting, were five in number.

The first related to the materials which compose the coal strata, their sources, relative distribution, and special conditions of deposition.

The second, to the structure of the anthracite district of Pennsylvania, the forces which uplifted its strata, and their effects on the beds of coal.

The third treated of the partial metamorphism of the strata, the laws of gradation in their composition and thickness, the debituminization of the coal and its conversion to anthracite, and the analogies between the anthracite region of Pennsylvania and the metamorphic coal basins of South Wales and Belgium.

The fourth discussed the erosive action of the floods which have swept over the surface of the Pennsylvanian coal fields, and the influence of the relative hardness and softness of the strata, under the action of the waters, in producing the existing features of terraces, slopes, and crests, so conspicuous in its topography.

The fifth division related to the physical statistics of the coal fields of the United States, to some estimates of their duration, and to views of the fittest economical adaptations of the several varieties of coal.

Under the first head, a comparison of the strata at numerous localities was made, in proof of the interesting general fact of the gradual thinning down, towards the north-west and west, of all the more exclusively land-derived deposits, the sandstones and conglomerates especially, and of a corresponding reduction in the coarseness of their fragments, with an equivalent progressive increase of the sea-derived materials in the same directions. The Carboniferous Red Shale Formation, a vast red mud and sand deposit of the ancient eastern coasts of the Appalachian sea, embraces in Eastern Pennsylvania the region of its maximum development, where it has a thickness of three thousand feet, not a single bed of limestone, nor one oceanic fossil to prove a mid-sea deposition; but when followed westward, these evidences of deeper waters gradually appear, and while the land-derived materials are acquiring an almost impalpable fineness, the sea-precipitated calcareous beds grow both more bulky and more replete in characteristic marine remains. So, likewise, the great conglomerate mass, which rests

immediately on the red shale, and is the lowest division of the productive coal measures, shows a like gradation, declining from a thickness of more than 1400 feet at its south-eastern outcrop, near the Schuylkill river, to 650 feet in spreading westward to the Susquehanna, and to not more than 100 feet in the escarpment of the Alleghany Mountains of Pennsylvania.

While this immediate floor of the coal measures is thus thinning down, the first or lowest coal seams progressively increase in size to a certain limit in the same westward direction, and even the main mass of the conglomerate itself, destitute in its south-eastern exposures of any true layers of coal, becomes a part of the productive measures by interpolation of valuable coal beds, thickening towards the west. It is a curious fact, in the horizontal distribution of these lower coal seams, that each coal overlaps the next subjacent one, further and further towards the south-east, as if the landward margin of every successive coal-producing forest or morass had encroached further in that direction than the preceding. This relation of the inferior anthracite beds, and a strictly analogous feature in the distribution of the lower bituminous beds, west of the Alleghany river, appear to point to a general law, and to imply not merely a gradual sinking of level of this whole region during the deposition of the coal strata, but such a rate of depression as to allow the carboniferous ocean to creep eastward upon the land *faster* than the land-derived materials could fill out its shores.

The north-westward enlargement of the lower coal beds to a productive size, in the usually barren conglomerate, confers upon the western extremities of the anthracite basins, particularly that of Shamokin, a more than average amount of coal, and imparts to them the somewhat peculiar feature of large and valuable beds, outcropping among the crests of the conglomerate mountains which encircle all these coal fields.

In proof of the dynamic conditions under which the strata were deposited, the very wide horizontal extension shoreward, as well as seaward, of individual strata,—the subdivisions of the conglomerate, for example,—their very gradual and uniform declension in one nearly constant direction, and the crushed and fragmentary state of the vegetable relics which they imbed, were all dwelt upon, to show that no local or circumscribed *estuary* or *deltal* actions could have attended their formation, but that they were thus largely spread by broad, general, and sudden displacements of the shallow waters of the carboniferous coast, disturbed into stupendous inundating billows of prodigious

transporting power, by the violent rocking motion of successive earthquakes.

Under the next general topic, or that of the structure of the anthracite basins, the flexures of the strata, or the anticlinals and synclinals, were shown to group themselves, as to direction, into two separate systems: one, a system of stupendous undulations, wherein the individual waves or flexures of the coast have an amplitude of many miles, and a length of nearly one hundred, with an average direction of their axes of about North 75 degrees East; the other a system of much narrower and shorter subordinate undulations, lying within the basins and on the backs of the anticlinals of the former system, and observing a mean direction, at least five degrees further from true east and west. This absence of parallelism in the two sets of flexures is especially conspicuous in the great middle anthracite region, or that of the Mahonoy and Shamokin basins, where the smaller anticlinals of the second system subdivide these main basins into a series of subordinate troughs, arranged with acute obliquity diagonally across them. It is a feature of much importance in giving ready access to the coal. These lesser, slightly oblique undulations, are evidently of the same class as the smaller flexures of other districts of the coal region of Pennsylvania, where they display a stricter parallelism with the great anticlinals. The deviation from such parallelism in the Shamokin and some other coal fields, is to be regarded as rather an apparent than a real aberration from the general law of coincidence in direction, and is easily accounted for by referring them to their own proper system of parent or larger undulations to which they are subordinate, and with which they are accurately parallel. These larger anticlinals do not themselves coincide in direction with those of like great dimensions within which their minor secondary flexures sometimes lie, and it is to the interference near their adjacent extremities of these two main systems of undulations that this partial interlacing of the lesser ones is to be attributed. Only the great primary flexures of the strata are regarded by the author as undulations affecting the entire ancient thickness of the earth's crust; the smaller and comparatively insignificant ones, which seldom have an amplitude of half a mile, are considered in the light of superficial foldings or corrugations, engendered in a secondary manner by the inability of the imperfectly compressible strata to conform themselves to the troughs and curves into which the crust has been undulated.

In connection with the next subject,—the partial metamorphism of

all the anthracite strata,—several new phenomena were detailed, indicative of the conditions under which the coal was converted from the bituminous form to the anthracitic. A regular and very delicate gradation in the composition of the coal is traceable in the direction from east to west, from the hardest and most compact anthracite, destitute of any vestige of bituminous matter or of carburetted hydrogen, to soft, flaming, and free-burning semi-anthracites, and semi-bituminous coals, within the limits of the anthracite basin, and to the most fusible and fattest bituminous varieties beyond those bounds in the valley of the Ohio river. In examining the physical properties of the coals in parallelism with this gradation in their chemical composition, all those varieties which display the intermediate grades of partial debituminization,—both the semi-anthracites of the western beds of the anthracite fields, and the semi-bituminous coals of the eastern margin of the great western bituminous basins,—show conspicuously, in the cleavage structure of the coal itself and its texture, distinct evidence of its having been softened and metamorphosed during the escape of its gaseous ingredients. A suite of specimens designed to illustrate the memoir, showed plainly the peculiar transverse or slaty cleavage structure, so significant of metamorphism in all strata susceptible of softening, and so indicative of the agency of a pervading high temperature. The anthracites are therefore to be regarded as varieties of coke, and the coking process may be considered as having been effected slowly and under an enormous pressure, the escape of the elastic gases from the substance of the coal being facilitated by its innumerable cleavage fissures. Under these conditions, it is obvious that even the whole of the volatile constituents of the coal might gradually pass away without the production of any of those cells or little vesicles which distinguish artificial coke.

The evidences of a general denudation and universal erosion of all the strata were next presented and discussed. In the anthracite region of Pennsylvania, where, from the great length, comparative narrowness, sharpness and parallelism, of the anticlinal waves, the various members of the coal series have been exposed at the time of their elevation to the cutting action of the retreating waters at high angles and in parallel and narrow belts, the great inequality in their firmness of cohesion or resistance to the erosive process, has caused the surface of each of the coal basins to be carved into the most regular and beautiful terraces, and intervening ridges and slopes, the soft and easily removed shales and coal seams forming the flats or stages on the hills and

mountains, and the hard and massive conglomerates and sandstones, the acclivities and the crests. In the bituminous coal fields of the great Appalachian basin, where the undulations of the rocks are wider and flatter, and the dips more gentle, and where, moreover, the inequality in the tenacity of the strata is materially less, these external traces of the denudation are expressed in a different set of features. The terraces and slopes are less conspicuously marked, and all the sculpture of the surface, though easily readable, and extremely beautiful, is marked by less abrupt transitions, finer lines, and more flowing curves. In both regions, and everywhere throughout the intermediate districts of the Appalachians, the erosion, though seemingly very energetic, was of the most diffusive character, and of apparently nearly equal intensity, and in no one definite direction. All the features of the topography, from the most salient to the most delicate, imply a general *running off* of broad uplifted waters towards all points from higher to lower levels, and not the violent transit of a great sheet or current from a given quarter, as so strikingly indicated in the later phenomena of the Drift.

In accordance with this topographical evidence of a diffused and forcible draining off of the waters at the close of the carboniferous period, are some very interesting features in the outcroppings of the softer strata of the anthracite basins. All the more flexible beds, the coal seams and their slates especially, are found to have their upper edges or outcrops,—even where overlaid by from five to ten feet depth of local angular diluvium,—turned invariably *down hill* to the extent of an inversion or curling over of the crop. This fact, brought to light by researches in quest of coal, the author has extensively verified.

Confirming these several proofs of the quaquaversal retreat of the denuding waters, are the marks of strong erosion on the more firmly cemented rocks capable of retaining its traces. The coarse silicious conglomerates of the mountain ridges enclosing the Schuylkill and the Shamokin coal basins, situated altogether south of the limits invaded by the later true Drift, show unequivocal traces of a general erosion of their surfaces—not in the form of *roches moutonnes*, but in the worn, and rounded, and smooth aspect of their prominent edges, without relation to any definite direction, also in certain water-worn, rounded excavations, analogous to shallow pot-holes, and even in the abrasion of their hard quartzose pebbles.

In the summary of the physical statistics of the coal fields of the United States with which the paper closes, the productive area of the

anthracite fields of Pennsylvania is stated, upon a careful review, to not much exceed two hundred square miles. In some vicinities, especially that of Pottsville, and of the western end of the Shamokin basin, where four productive coal seams lie within the conglomerate, the aggregate thickness of pure merchantable coal is estimated at about one hundred feet, but perhaps seventy feet is a more just average for the whole anthracite region. Thus there are localities where a single square mile of soil covers at least fifty millions of tons of fuel.

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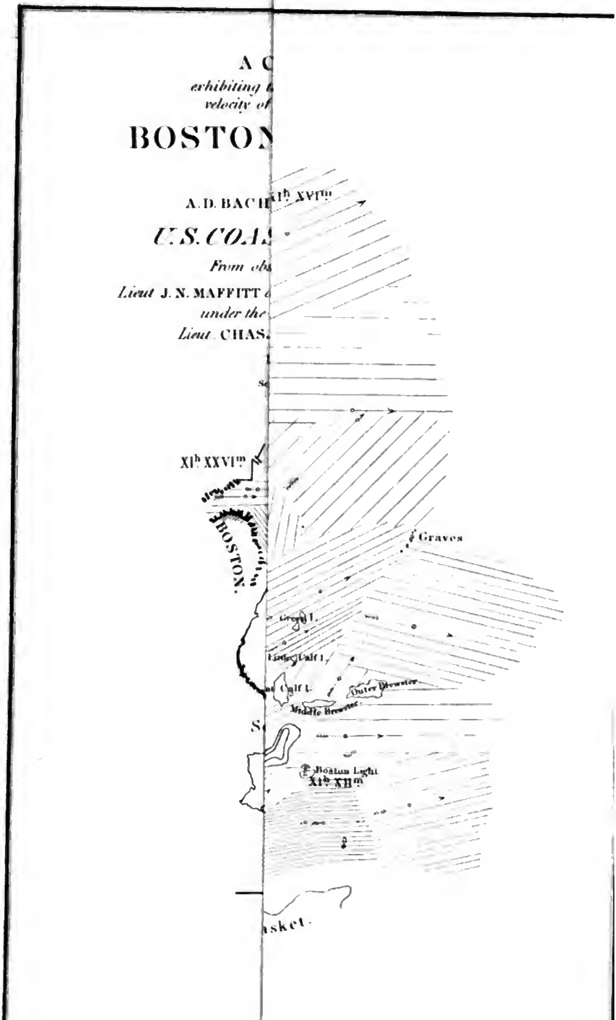
5. Method used in the Coast Survey of showing the Results of Current Observations. By A. D. Bache, Superintendent.

This method, while it is original, may not be new, though I am not aware that the system has been followed by others. It has some analogies with the representations of Berghaus, but is different essentially from them. The principle, and an application to the very elaborate current observations made in Boston Harbor, under the immediate direction of Lieut. Chas. H. Davis, by Lieut. J. N. Maffit, are shown in the diagrams now presented to the Association.

Observations of the direction and velocity of the currents at and below the surface have been extensively introduced into the hydrography of the Coast Survey; their importance needs no remark. For most of the details of arrangement and suggestion in regard to the earlier observations, I am indebted to my brother, the late Lieut. Geo. M. Bache, U. S. N. For much of the success of execution to Lieuts. Chas. H. Davis and Carlisle P. Patterson, U. S. N., who have taken unwearied pains in the matter. For currents below the surface there is still wanted some sure and easy method of determining both direction and velocity, especially the latter elements.

Observations being multiplied at different periods of the current from slack water to slack water, they are projected upon diagrams showing at a glance the direction and velocity at any particular station. The average of the results is usually obtained by inspection. Dividing the intervals between slack waters into quarters, we give the mean results for those periods in a table, and place usually upon the chart arrows indicating the direction or set, and write at the extremity numbers showing the velocity or rate in miles per hour. In case of the observations made in Boston Harbor, the results were so unusually numerous that the lines of direction were confusing to the eye, and the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exhibiting
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BOSTON
A. D. BACH
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Lieut J. N. MAFFITT
under the
Lieut. CHAS.
BOSTON.
Graves
Calf I.
Outer Brewster
Middle Brewster
Boston Light

connection between the results was very difficult to seize, though, from the pains taken, the motion of the water was traceable in nearly all its peculiarities, from the entrance through the tortuous passages among the islands, alternately narrowing and expanding to the city wharves.

In the current chart now brought before the Association, the direction and force of the currents are represented by lines, the distance between which is inversely as the rate in miles per hour. The scale was obtained by making the least velocity correspond to nearly half an inch and the greatest to .06 of an inch. The reciprocals of the number of miles per hour are represented by tenths of inches in the diagram, currents of 0.2, 0.5, 1, 1.5, 2 miles per hour being represented by lines parallel to their directions, and distant 0.5, 0.2, 0.1, 0.67, 0.05 of an inch. The chart is on a scale of $\frac{1}{20000}$. The representation on one of the diagrams corresponds to the *flood*, and in the other to the *ebb*, referring to the motion of the current from slack water to slack water, and not to the tide or rise and fall of the water. If the current stations were very numerous, the straight lines tangent to the curves of motion of the water (set of the current) would become curves.

It is easy for the navigator to seize the relations of the currents he will meet, even by these tangent lines; to avail himself of the knowledge this imparts of the direct, lateral, and eddy currents, to avoid danger or to secure advantage.

The cause of the change of directions and velocities is in most cases well marked, the generalizations presented to the eye being connected with the form and position of the land above or below the water. In some cases, the chart indicates that the observations should be repeated, and in others that, numerous as the stations are, they should be still increased in number. Its adoption will probably thus react to improve the observations upon which it is founded.

On the Velocity of the Galvanic Current in the Telegraph Wires. By B. A. Gould, Jr.

One of the most prominent and interesting of the numerous questions which are at the present moment pressing themselves upon the attention of scientific men, is to determine the velocity with which signals are transmitted along the wires of the electric telegraph; and this question has already enlisted the interest of several investigators on both sides of the Atlantic.

The ingenious experiment* of Prof. Wheatstone in 1834, tended to confirm the general opinion previously existing, that the velocity with which the electricity was transmitted by a metallic conductor was so enormous—so immense, indeed, compared with all other velocities known to us, excepting that of light—as to warrant the assumption of our incapacity to determine it. On this account, Wheatstone's elegant experiment obtained for its author the more distinction, and for his results the greater confidence. One of these results, as announced by him, was, that the velocity of electricity through the copper wires used, was indeed appreciable,—but exceeded that of light through the planetary space,† that it could not be less than 288,000 miles in a second, while light traverses about 196,000 during the same time.

The telegraphic observations, instituted under the immediate direction of Mr. Walker, by the U. S. Coast Survey, for determining the differences of longitude between remote stations in the United States, led to a very unexpected result,—viz.: that to obtain the greatest harmony among the several observations, a small correction must be introduced, depending on the relative distances between the telegraphic stations. No explanation of this phenomenon offered itself, excepting the hypothesis, suggested‡ by Walker, and communicated by Prof. Bache to the Am. Phil. Society in March, 1849, that the time elapsing during the passage of the signals between remote stations was much more considerable, and the velocity, consequently, less than had been before imagined.

Since Walker's results were first published, the subject has engaged the attention of numerous astronomers and physicists in Europe and America, among whom Mitchel, Fizeau, and Steinheil are conspicuous. The subject belongs in itself far more properly to the domain of physics than to astronomy, but its special bearing upon the problem of longitude, and the manner in which it has forced itself upon the consideration of astronomers, have made it incumbent upon them to enter into a full discussion of the subject.

While in Washington in the month of February last, I accepted with pleasure an invitation from Mr. Walker to take part in an experiment on a very large scale, for which he had been long engaged in making preparation in behalf of the Coast Survey, and from which he anticipated results so ample as to put an end to the controversy. The Seaton

* Phil. Trans., 1834, p. 583. † Phil. Trans., 1834, p. 591.

‡ Proc. Am. Phil. Soc., v. pp. 76. Astr. Nachr., xxix. 54.

Station of the Survey in Washington, north of the Capitol, and the city of St. Louis, were connected on the 4th February, in one colossal galvanic circuit, and but for the damage occasioned by a storm on the same day, the circuit would have extended even to Dubuque, in the Territory of Iowa, a distance of some 1500 miles.

My friend Dr. Bache, Superintendent of the Survey, did me the honor, subsequently, to place at my disposal all the materials derived from the experiment, with an invitation to investigate the question independently. I have accordingly devoted some labor to their discussion, and propose to state the conclusions to which I have arrived.

It may be permitted me, however, to add, that I submit them with diffidence, for various reasons—not the least of which is, that some of these conclusions are at variance with the known views of scientific friends, on whose judgment I place great reliance.

As but few have probably followed all the investigations on the subject, a large proportion of which have been published within a twelvemonth, it may be well for me to recapitulate to some extent what has already been done. Thus, before detailing the results of the experiments of Feb. 4, I will endeavor to state the position of the theory before the discussion of those results. A peculiarity of the question consists in its intimate relation with the theory of electrical force, and the nature of conduction. This relation is so intimate as to render it almost impossible to discuss the subject without making use of phraseology derived from hypothesis. This may, however, be done with perfect safety—so long as we remember that such phraseology is only used for convenience of expression. If, then, I avail myself of such expressions as "electric current,"* for instance, "circulation," "wave," or similar phrases derived from hypothesis, plausible or otherwise, I will ask to be considered as using the terms in a metaphorical rather than in a literal sense. A simple idea may thus be expressed in a single word, while a carefully guarded circumlocution would otherwise be necessary, to distinguish "between the transmission of the effect and of the supposed fluid by the motion of whose particles that effect is produced."†

The wires of almost all the telegraph lines in the United States, and of all, I think, which have been used in the experiments for velocity (with a single exception), are of iron, and of the size known in trade as No. 9, being about three millimeters in diameter. We have every reason for believing that the velocity with which electricity is conducted

* Faraday, Researches, i. pp. 81, 148.

† Id. i. p. 526.

varies* with the conducting power of the medium, and should therefore naturally anticipate that this velocity would be found greater in copper than in iron. Wheatstone used in his experiment copper wire $1^{mm}.7$ in diameter. Now the conducting power of iron is, according to Lenz,† Riess,‡ and Pouillet,§ who determined it by different methods, less than $\frac{18}{100}$ that of copper at 0°.C. And when we take this circumstance into consideration, it appears a sufficient ground for believing that the results of Walker and Wheatstone are not inconsistent with each other—even without reference to the fact that the one used the galvanic current, and the other machine-electricity of the highest possible tension.

Walker's first results,‖ derived from the longitude operations of the Coast Survey, led him to the opinion that the velocity with which the signals passed between Cambridge and Washington on the night of Jan. 23, 1849, was 18,690 miles a second, with a probable accidental error of 1000 miles.

On the night of Oct. 31, 1849, a series of experiments was made for the express purpose of determining the time needed for the transmission of signals. The results are published¶ in No. 7 of the *Astr. Journ.*, with a detailed account of the methods which he used, and an analytical investigation of the effects of those circumstances which could interfere with the accuracy of his results. The measurements of all the registers gave him for the velocity on that night 16,000 miles a second,—differing less than 1900 miles from his previous result, and tending in general to confirm it. The final result at which he arrived was the general theorem,—that a signal given by breaking or closing the galvanic circuit at any point, was observed at other points on the circuit after intervals proportionate to their distance from the place where the signal was made,—and corresponding to a velocity of from 16,000 to 19,000 miles a second. Later experiments with the chemical telegraph are described** in No. 14 of the *Astr. Journ.*, and give a still less velocity.

Prof. Mitchel, of the Cincinnati Observatory, dissents from the view taken by Mr. Walker, and attributes the results obtained by him to the effect of various sources of error and uncertainty in the methods which Walker has used. He devised a special and very curious apparatus for

* Faraday, Researches, i. p. 423.

† Pogg. Ann. xlv. p. 109.

‡ Pogg. Ann. xlv. p. 20.

§ Pouillet-Müller, Lehrb. der Physik, ii. p. 186.

‖ Proc. Am. Phil. Soc. v. p. 74.

¶ Astr. Journ. i. p. 49.

** Astr. Journ. i. p. 105.

investigating the question,—and with the ingenuity and mechanical skill for which he is so eminent, constructed it at the Cincinnati Observatory, and made a large series of interesting experiments on the telegraph line between Cincinnati and Pittsburgh.* Prof. M.'s view of the matter is, that after a signal is given by closing or breaking the galvanic circuit, an appreciable time elapses before this signal is communicated to any other station, and that it is then received by all simultaneously. He considers this in connection with the details of his experiment to indicate that two fluids circulate in opposite directions between the poles of a battery, but that neither makes its influence perceptible until complete circulation of each† has taken place from pole to pole. The velocity of this circulation Prof. Mitchel infers to be about 30,000 miles a second.

Finally, Fizeau and Gounelle, in a paper published‡ last April, in the *Comptes Rendus* of the French Academy, express a want of confidence in Walker's results, and describe experiments of their own, made on a circuit of 374 miles—a distance which they characterize as enormous, although the circuit used in the first Coast Survey experiments of Jan., 1849, extended through 550 miles of wire between Cambridge and Washington, and terminated at stations 380 miles apart in a geodetic line. These gentlemen used a method totally different from either Walker's or Mitchel's, and found that it gave them 62,000 miles a second, as the velocity in an iron wire, 4 millimeters in diameter, and 110,000 as the velocity in a copper wire, $2^{mm}.5$ in thickness. Their method is not unlike that of Jacobi's experiments§ at St. Petersburg in 1838 for the determination of the time necessary for the development of a galvanic current. The experiments appear to have furnished no data for an inference as to whether a signal is necessarily communicated to the several parts of the circuit after intervals proportionate to the distance, or not.

Such is the present state of the theory, to the best of my knowledge,—and it will be observed that the views in the most fundamental points are far from unanimous. Still greater differences of opinion exist in regard to the more special questions.

The experiments of Feb. 4 have, it appears to me, furnished ample materials for arriving at a decision on very many of the vexed points;

* Astr. Jour. i. p. 13. Astr. Nachr. xxx. p. 325.

† Faraday, Researches, i. pp. 518, 520, 521.

‡ Comptes Rendus, xxx. p. 437.

§ Pogg. Annalen, xlv. p. 281.

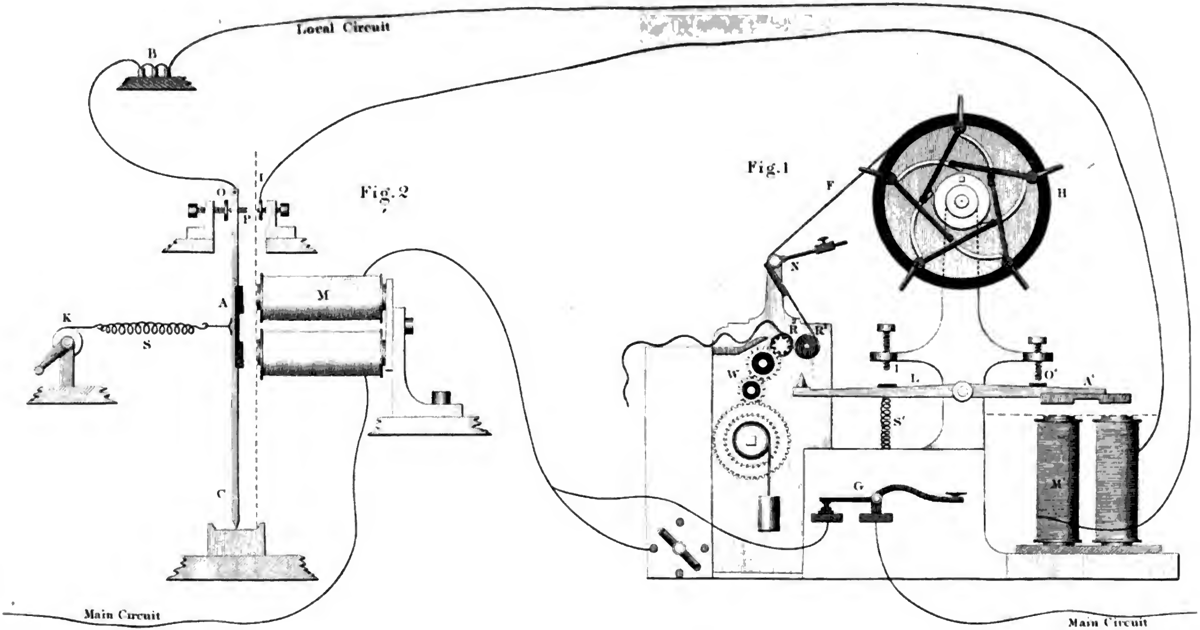
and without further introduction, I shall proceed to consider the nature of those experiments, the sources of error, and the amount of error introduced by them. Mr. W. has already done this very elegantly in the memoir* to which I have referred; but I will also briefly consider the subject here. It will then be more easy to enunciate the questions, to which a discussion of the experiments between Washington and St. Louis ought to furnish a reply.

In the electro-magnetic telegraph offices, using Morse's patent, signals are communicated by the magnetization of a piece of soft iron, in the form of a horse-shoe magnet, and placed within a helix of wire. When the current flows, the iron becomes magnetic, and attracts an armature placed before it and connected with a graving tool. An endless fillet of paper is kept moving beneath the graver, by means of clock-work, so that the fillet is unmarked when no current exists in the wire, but is grooved by the action of the tool while the circuit is closed.

Fig. 1 (see Plate) represents the recording apparatus and its adjustments. M is the magnet, A the armature, which is held back by the spiral spring S, while the current is not flowing, but is attracted as soon as the circulation of the current through the helix of wire has rendered M magnetic. The tension of the spring is regulated by a key. H is the reel for the paper fillet which receives the record; N a guide for directing and compressing the paper upon the roller R'. The roller R receives motion from the wheelwork W, and communicates it to the paper fillet F. While the armature is attracted, the indentor at the end of lever L presses upon the fillet, marking a groove whose length (the motion being equable) is proportionate to the duration of the electro-magnetism in M. The armature is never allowed to touch the magnet, but the proximity of its approach is limited by the stop I. This and the stop O, which limits the outward motion, are capable of adjustment by screws. The distance traversed by the armature in passing from the outer to the inner stop is technically called the *pass*.

As the wire may be coiled into any number of helixes in the course of a circuit, there is no practical limit to the number of intermediate stations, each of which may receive all the signals quite as well as the terminal station. But since the intensity of the current decreases rapidly as the distance from the battery increases, in consequence of

* Astr. Journ. i. p. 105.


Local Circuit
B
O
I
P
Fig. 2
M
A
K
S
C
Main Circuit
Fig. 1
F
H
N
R
R'
W
L
I
O'
A'
S'
G
M'
Main Circuit

the imperfect insulation of the wires, Morse's ingenious application of a local circuit is used, that these signals may be distinctly registered.

Fig. 2 represents a station on the main line. The signification of the letters is the same as in fig. 1, the key K regulating the spring. The armature-beam C is connected with one electrode of the local battery, the other being connected with the metallic inner stop I. An attraction of the armature closes the local circuit, on which is the recording apparatus, and thus a very weak current may suffice to close the circuit of the local battery, which may be regulated to any degree of intensity required.

Let us now suppose that at one extremity of a long circuit the pendulum of a clock is made to interrupt the current for a moment at each oscillation, the circuit being restored immediately afterwards, as before. We shall have on the registers of the several stations a series of lines alternating with pauses or blank spaces,—*dots*, as they are called, in the technical phraseology of the telegraph.

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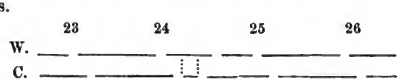
If the fillets were moved forward with a uniform velocity by the machinery of the registering apparatus, and no changes occurred in the intensity of the current in the wire, each of the lines upon any one fillet would be of the same length,—as also would each of the pauses. And if the velocity given to the fillets at the several stations were the same, the lines representing seconds would be of one length on all the registers. This last we cannot expect, nor is it in any manner necessary, for it is an easy matter to reduce all the registers to a uniform scale. But that the motion of each single fillet be uniform, is very desirable. To obtain a near approximation to such uniformity requires machinery far more delicate and more carefully constructed than that which is in use in the telegraph offices, and with which the experiments of the Coast Survey have been made. Such apparatus has been devised and is constructing by Mr. Boyden, of Boston, and by Mr. Bond, of the Cambridge Observatory. It forms also an essential part of the apparatus of Prof. Mitchel. Yet, as we are in fact dependent on the rate of the driving machinery of the registering apparatus only for a single second in each case, the error from this source is reduced to that arising from the alteration of velocity between the beginning and end of the same second. This is very small, except in extraordinary cases, and we may assume that all errors of this kind eliminate themselves when we use the mean of a number of cases, instead of isolated observations.

At each station on the line of telegraph, during the experiments for

velocity, a contrivance is introduced into the wire circuit, of such a kind, that the circuit may be broken by pressing upon a key, which is restored by a spring to its original position as soon as the pressure is removed. One of these "break-circuit" keys is represented at G (fig. 1). It is thus in the power of the operator at any station to break the circuit whenever he chooses, and in this way to communicate at once with all other stations on the line.

After a galvanic battery and the circuit-breaking clock have been connected with the line, we have at each station a clock scale, that is, a series of lines, separated by short pauses, each pause corresponding to the beginning of a second, whose duration is denoted by the length of the grooved line. In the clock of this kind at Washington, that pause is omitted which corresponds to the beginning of each minute, and this enables us to identify on a fillet from any station the clock-pause made at a given second. But whatever the velocity with which the clock-pause is transmitted,—or, in other words, however great an interval may elapse after the pendulum has broken the circuit before the pause is recorded on the register-fillet of a distant station,—we have as yet no means of detecting it; for in comparing the registers made at different places, this interval will (other things being equal) be the same for all the pauses, and therefore remain undiscovered.

Let us, however, imagine that while the clock is graduating its scale on all the registers as before, an arbitrary signal is given by breaking the circuit at some distant station. The comparison of the several registers will thus enable us to register the interval which elapses between the giving and the receiving of a signal. To fix our ideas, suppose the clock to be at Washington, the arbitrary signal to be made at Cambridge, and the time requisite for the transmission of a signal between the two cities to be the thirtieth of a second. Then the clock-pause will be registered at Cambridge $\frac{1}{30}$th of a second after it took place and was recorded at Washington, and the arbitrary signal-pause will be recorded at Cambridge as soon as it is made, or $\frac{1}{30}$th of a second *before* it reaches Washington. We shall thus have the interval between the signal-pause and the preceding clock-pause longer at Washington than at Cambridge, and the excess on the Washington register will measure twice the time consumed in the transmission of the signals between the two stations.



In order to avoid accidental errors, the mean of many measurements may be used,—and half the mean excess of the interval between a clock-pause and the next succeeding signal-pause gives us the time occupied for the transmission of a signal.

Such is the general method;—let us consider the possible sources of error and their probable influence. Every clock-pause and every signal-pause amounts in reality to a combination of two signals, distinct from one another, and subject to different general laws. The one consists of the electrotome, or breaking of the circuit: the other is the restoration of the circuit, or *electropœa*,—to use Mr. Walker's word.* We can consider the intervals between the signals as being the same as the intervals between the electrotomes, or beginnings of the pauses,—between the *electropœas*, or beginnings of the lines,—or as being more properly measured from the middle of one pause to the middle of the next. Mr. Walker has used the latter, the mean of the electrotome and electropœa readings.† For my own part, I prefer to use the electrotomes alone.

The only signal that can be made in a closed circuit is of course an electrotome. We break the circuit by tapping on the key. As soon as the finger is removed, the circuit is closed again by a spring, and an electropœa signal thus made. Do these two signals travel with the same velocity? If they do not, the clock-pauses must increase or diminish in length, as they are successively recorded at more remote stations. They must become elongated if the electrotome signal travel the faster, and contracted if the contrary. This question is an important one, but its discussion may be postponed for a while, as the length of the recorded pause is dependent on many other circumstances, and especially on the adjustments of the registering apparatus, which are not only very different at different stations, but appear to have been continually changed at the same station during the evening by the telegraph operators in charge of the registers. As far as I feel warranted in forming an opinion, I incline to the belief that these two kinds of signals traverse the circuit with equal speed,—or, to express the same idea in a different form, that induction of the electrical state in the successive molecules of the conducting medium, requires neither more nor less time than that required for a molecule to be restored to a condition of entire electrical equilibrium. The better to fix one's ideas in thinking on the

* Astr. Journ. i. p. 51.

† Proc. Am. Phil. Soc. v. p. 76. Astr. Nachr. xxix. p. 56.

subject, some hypothesis is desirable, which shall be capable of explaining all the phenomena; and I have accustomed myself to consider the propagation of electric polarity through a medium, as taking place* by the inductive force exerted upon each successive molecule† by the adjacent one.

If we consider the telegraph-wires as composed of a series of contiguous elements, an electrotome taking place at any point puts an end to the state of electrical tension in the nearest element on each side. The electrical equilibrium being restored in these elements, no force exists‡ to continue the disturbance in the succeeding ones, and so on. An analogy to the propagation of an electropœa, or electric perturbation, may be drawn from the known laws of magnetism. If we have a curved rod of soft iron forming a little less than an entire circumference, and fill the gap with a bit of magnetic iron, the whole circle becomes magnetic. Although the period required for the transmission of this force is so short as to defy all attempts to measure it, there can be no doubt that a finite time intervenes between the introduction of the magnet and the magnetization of the most remote part of the ring. When it is necessary to conceive some actual mode of transmission of signals, I am accustomed to represent it to myself in this way. The experiments of Wheatstone show that the electric spark took place at the same moment at points equidistant from the battery on the two sides, and latest at the point midway between the poles,—which is in perfect conformity with the hypothesis offered.§ For the present, then, we may assume that electrotome and electropœa signals are transmitted with equal velocity; and, in a circuit composed of but one medium, in both directions.‖

The circuit being suddenly closed, the electric disturbance is propagated through the telegraph-wire with a certain speed, and on reaching

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* Faraday, Researches, i. § 1677.

† Ibid. § 1700.

‡ Ibid. i. §§ 1671, 1686.

§ A very strong analogy to a galvanic circuit is afforded by the charge and discharge of an electrical battery "by cascade." The first jar being connected with the prime conductor of an electrical machine—a series of jars, where the interior of each successive one is connected with the insulated outer coating of the preceding—furnishes an imitation of a broken circuit connected with a battery. A connection made between the exterior of the last jar in the series and the interior of the first, either through a discharging rod or through the ground, corresponds to an electropœa or *make-circuit.* The difference between the charge of the first and of the last jar represents the resistance to conduction.

‖ Faraday, Researches, i. §§ 516, 1630.

a registering station traverses the helix, imparting magnetic force to the encircled horse-shoe of soft iron. The armature traverses the pass and closes the local circuit. The same phenomena are repeated in the local circuit, and the graver attached to the armature-lever makes its mark upon the paper. All these processes require time, and the absolute interval elapsed between the electropœa signal and its record on the paper fillet is thus very considerably larger than that due to the time of transmission of the signal along the wire. The difference consists—

1st, of the *induction-time*, or time which elapses after the current is established in the helix, before sufficient magnetic power has been induced in the iron to overcome the tension of the spiral spring, and move the armature.

2d, of the *pass-time*, or time required by the armature for traversing the pass, and closing the local circuit.

3d, of the time of transmission of the electropœa in the *local* circuit.

4th, of the induction-time for the local circuit magnet.

And 5th, of the pass-time in the same.

If the adjustments of the local circuit are good, and are not changed during the experiment, and if the action of the local battery is constant, the last three quantities remain the same for all electropœa signals during the experiment, and exert no influence except in lengthening all the registered pauses by a very small quantity. This has usually been the case, and simplifies the problem very considerably, leaving us* only three unknown quantities to deal with,—the time of transmission, and the induction and pass-times of the main circuit.

The record of the electrotome signal is not so much exposed to accidental disturbance. The moment that the current ceases in the helix, the enclosed iron begins to lose its magnetic condition, and as soon as it has parted with its magnetism sufficiently to allow the tension of the spiral spring to draw off the armature, the local circuit is broken. The outward pass-time has no influence upon the register. The only quantity to be considered is the time elapsing between the cessation of the current in the helix, and the loss of magnetism in the iron. This interval is the converse of the induction-time, and might, therefore, be denominated the *eduction*-time.

The same that has been remarked of the corrections in the case of an electropœa, holds true with regard to the propagation of an electrotome

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* Walker, Astr. Journal, i. p. 52.

through the local circuit, and the eduction-time in its electro-magnet. The local circuit adjustments being well made, and continuing unchanged, and the intensity of the battery remaining constant, the error from these sources is the same for all electrotomes and electropœas.

The chief source of error lies, then, as will be observed, in the pass-time of the armature of the receiving magnet, and may be totally avoided by using the electrotome signals only,—as it exercises no influence whatever upon these. It is for this reason that I have preferred to use the electrotome readings only, in preference to a mean between them and the electropœas. The pass-time is a very considerable quantity when compared with the small numbers with which we have to deal in these investigations.

The adjustment of the pass to the most convenient value is made by the operator, by means of the screws at I and O (figs. 1 and 2.) The force with which the armature is attracted may be considered as varying nearly in the inverse ratio of the distances,* except when in very close proximity to the magnet. The distance within which it may be allowed to approach the poles of the magnet is limited by the condition that the tension of the spiral spring must be strong enough to withdraw it as soon as possible after the circuit is broken, and yet not so strong as to delay its starting to return, when the circuit is closed again. The pass is then made as short as is practicable, allowing sufficient play to the armature for all the lines and pauses made on the fillet by the graver to be distinct and sharply defined. The following considerations have guided me in making an estimate as to the pass-time in the experiments under discussion.

If we assume the intensity of the attraction exerted by the magnet on the armature to be equal to gravity at the earth's surface, the following numbers give the pass-time for different lengths of the pass.

Length of Pass. Millimeters.	T Seconds.	Length of Pass. Millimeters.	Time. Seconds.
2·50	0·0226	1·25	·0160
2·25	·0214	1·00	·0143
2·00	·0202	0·75	·0124
1·75	·0189	0·50	·0105
1·50	·0175	0·25	0·0086

As far as I can judge, the usual pass in the main circuit *receiving magnet* is between $1^{m \cdot}{}_{m}75$ and $0^{m \cdot m}50$, which would, on our present

* Channing, Davis's Manual of Magnetism, pp. 150–155.

assumption, give the pass-time 0·019 and 0·010. An attractive force, four times that of gravity, would give the half of these numbers. But this mode of viewing the subject is a very rough one, for as the attraction varies with the distance, the initial force (which exercises the greatest influence upon the time of passage) would be much less when the length of pass is greater,—and the tension of the back-spring also operates to increase the pass-time very considerably. I incline to the belief that the average pass-time in the experiments of Feb. 4, was about 0·03. Mr. Walker, on the contrary, estimates it at $\frac{1}{15}$th of a second—more than twice as much. Had we any means of measuring the absolute duration of the interruption of the circuit by Mr. Saxton's clock, it would be of great service in aiding us to deduce the pass-time in many cases. In all conclusions drawn from the telegraph fillets alone, the pass-time and the induction-time are inseparably combined. Denoting the several intervals by their initials, we have for the length of the *recorded* clock-pause, which we will indicate by a *capital* letter, $C = c - e + i + p$.

As a first hypothesis, we may assume what appears not improbable from other considerations, that e and i are equal. Then subtracting the length of the interval during which the circuit was actually broken by the clock, the remainder would be the *pass-time.* For this purpose Professor Bache, at my solicitation, requested Mr. Saxton to form some estimate, if possible, of the interval between the breaking and closing of the circuit at each clock-pause. Mr. Saxton expressed himself unable to do this, which is much to be regretted. It had appeared to me that careful observations of the angle with the vertical, made by the pendulum at the time of striking the tilt-hammer, might justify some precision in forming an estimate. The pass-time, thus deduced, would in its turn enable us to examine into the propriety of our hypothesis regarding the equality of the *induction* and *eduction*-times. This is supported at present only by the result, in which I feel some confidence—that the velocity of the electrotome and electropœa signals is the same. The latter is, however, inversely proportional to the sum of all the induction-times of the molecules on the line of wire,—the former to the sum of their eduction-times. If these are equal for the wire, the inference is natural that they are equal for the iron inclosed in the helix, if this be as soft as the iron of the wire.

Such speculations are indeed loose, and would not be warranted, were data within our reach for an accurate investigation of the question. But as the problem now presents itself to us, everything appears inter-

esting and worthy of considerstion which affords opportunity even for a presumption.

In the apparatus used by my friend Prof. Mitchel, he considers* that he is able actually to measure the length of the armature time, which is the sum of the induction and pass-times. The method which he uses is, however, incapable of being applied to the record made with Morse's instrument.

In an earlier part of my remarks, I deferred the discussion of the question whether the electrotomes and electropœas traveled with the same velocity, and yet have just now expressed the opinion that they did. The opinion was formed as follows:

We have the length of the recorded clock-pause, $C=c-e+i+p$, and of the recorded signal-pause, $S=s-e+i+p$.

If we take the difference of these, the three unknown quantities disappear, and we have $C-S=c-s$, the difference of the real duration of the pauses equal to the difference of their recorded duration. The only assumption here made is that the induction, eduction, and pass-times, are the same for both pauses,—an assumption quite warrantable, if we compare each signal-pause with the clock-pause nearest it. They will then rarely be more than a quarter of a second distant from one another, and the values of the corrections are not likely to change appreciably within so short an interval. I have formed a table of the differences in the length of the same signal and clock-dots, for six different telegraph stations on the line between Washington and St. Louis, and, from the discussion of more than a hundred different signals, find that the mean of these differences is almost identical for all the stations.

The intensity of the current, even at the same place, is quite unequal, owing to various influences, among which are thermometric and hygrometric changes in the atmosphere, which affect the insulation and the conduction; want of constancy in the batteries, whose activity varies very much with variations of temperature; want of homogeneity in the substance of the plates; the continual formation† and flaking off of coatings of oxyd, and many other circumstances. We have already seen that the pass-time varies with the intensity of the electro-magnetism in the iron, and consequently‡ with the intensity of the current,—and can therefore infer that the different clock-pauses would not appear of the

* Astr. Jour., i. p. 16.

† Faraday, Researches, i. § 1114.

‡ Fechner, Schweigger's n. Jahrb., ix. pp. 274, 316. Lenz and Jacobi, Pogg. Annalen, xlvii. 233. Bull. de l'Acad. de Pétersburg, vol. iv.

same length even on the same registers. That they do not, may be inferred from the following table, which gives the mean value of clock-pauses at each station for each one of five successive minutes.

MEAN LENGTH OF CLOCK-PAUSE.						
	S. S.	W.	P.	C.	L.	St. L.
8h.47m.	0s·072	0·068	0·055	0·095	0·096	0·092
48	0·091	0·050	0·067	0·090	0·093	0·100
49	0·094	0·065	0·055	0·084	0·125	0·110
50	0·103	0·071	0·059	0·081	0·103	0·118
51	0·100	0·078	0·055	0·081	0·093	0·102
Mean.	0·091	0·067	0·058	0·087	0·104	0·100

It will here be observed that the variations follow no general law whatever, the length of the pause decreasing at one station while it increases at another, and being in no way proportionate to the distance from the clock-station, nor, so far as I can judge, to the strength* of the local batteries.

The pass of the Pittsburgh register was too small,—so small as to render the register very indistinct and difficult to read, and it will be observed that the clock-pauses are the shortest on the Pittsburgh register. The record upon the Cincinnati fillet was the most distinct of all, owing in part, however, to the finer quality of the paper. Clock-pauses occur of every length, from 0s·04 to 0s·16. I believe that the greater part of the irregularities are due to causes much less occult than those we have been considering, and chiefly to irregularities and imperfections in the mechanical part of the registering apparatus.

Bain's galvano-chemical telegraph affords an opportunity of trying the experiment under circumstances totally independent of the pass-time, which has proved so troublesome in the experiments made with Morse's apparatus. The paper on which the record of Bain's telegraph is made, is colored with a solution of ferrocyanate of potash. It rests upon a revolving metallic disc connected with one pole of the battery, while a needle or finely pointed wire connected with the other pole, trails on the upper surface. When the circuit is closed, the current passes between the needle and the metallic disc, coloring the paper in its passage by the partial decomposition of the ferrocyanate. Walker has made a series of experiments with this telegraph between Boston and New York.† But although the pass-time is here avoided, other

* Pouillet, Comptes Rendus, iv. p. 272.

† Ast. Journ., i. p. 105.

sources of error are introduced. The times* of delay and of persistence in the chemical action take the place of the induction and the eduction-times in the electro-magnet, and a new and serious difficulty presents itself in the spreading of the lines which have undergone chemical action.

The character of the electro-chemical telegraph allows no local circuit, in the technical sense of this term, and indeed it is unnecessary, for a current capable of closing a local circuit would suffice to act with energy on the very sensitive salt with which the paper is saturated.

But in the experiment for velocity, it is indispensable that a record be kept at the two terminal stations, at least; and the closing of the circuit, in order to record at one station, necessarily diverts the current from the other. With a powerful battery a record may be successfully kept at stations no further distant than New York and Boston; but if we use a line as long as that between New York and Buffalo, a battery so powerful as to burn a hole through the paper at one terminus, cannot record signals at the other, when the circuit of the signal-station is closed.

Mr. Bain has ingeniously remedied this difficulty by using a short circuit at the signal-station, which is closed by the same key that closes the main circuit. Yet even this still introduces a new source of uncertainty. A curious result which Mr. Walker has deduced† from his experiments consists in the fact that the discolored lines, which measure the duration of the current, are longer at the station where the signals are given, than at the other extremity of the line. An inference might be drawn from this, opposed to that which I have drawn from the St. Louis registers, concerning the equality of velocity for the electrotomes and electropœas; but it appears to me more probable that the circumstance is due either to the difference of circuit or to the superior intensity of the current, and consequently, of its chemical action at the signal-station. The question remains open for discussion.

The experiments of Fizeau and Gounelle, in France, described in the *Comptes Rendus* for April last, were made on an entirely different principle, and conducted them to a series of inferences which they give in detail. The published description‡ of their method is very obscure, and the data on which their inferences are founded are not fully given. Their method appears to be in some degree analogous to the very ele-

* Proc. Am. Assoc., 1849, p. 189. Ast. Journ., i. p. 108.

† Ast. Journ., i. p. 106.

‡ Comptes Rendus, xxx, p. 439.

gant experiment* by which Mr. Fizeau had previously measured the velocity of light between Paris and Montmartre. A wheel, whose circumference consisted of 36 divisions, alternately of wood and platinum, revolved in contact with the edges of insulated plates of platinum, each pair of plates forming a distinct circuit-breaker. The driving machinery of the wheel was connected with an apparatus for measuring the velocity of rotation ; and the effect of different velocities and different circuits was observed upon the galvanometer.†

The velocity of the current which they deduced from these experiment was, as I have already mentioned, 63,200 miles per second in iron wire 4 millimeters in diameter, and 110,000 in copper wire, $2\frac{1}{2}$ millimeters in thickness.

They inferred, moreover,

That the two electricities were propagated with the same velocity.‡

That the tension of the electricity has no influence on the velocity.

That the velocity does not vary with the section of the conducting material, but only with its nature, and then not in the ratio of the conductive power.

That the discontinuous currents "experience a diffusion, in consequence of which they occupy a space greater at the point of arrival than at the point of departure."

This latter result is in direct opposition to the experiments of Walker with the chemical telegraph, by which, using a method of determination indefinitely less complicated, he found the length of duration of the electric currents to be less at the more remote station. According to Fizeau,§ the electropœa is propagated more rapidly than the electrotome ;—according to Walker,‖ less rapidly. Fizeau used a circuit 374 miles in length, and Walker one of 250 miles of wire, whose two extremities communicated with the ground about 190 miles apart.

The Coast Survey experiments of Feb. 4, on the line between Washington and St. Louis, 1045 miles distant by the wires, and 742 in a geodetic line, lead me to suppose, as already stated, that the velocity of the two signals is the same, and (other things being equal) the pauses would be of equal length, as recorded at all the stations on the circuit. I am inclined to attribute Walker's results to the more intense

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* Comptes Rendus, xxix. pp. 90, 132.

† Becquerel, Traité, v. p. 275.

‡ Faraday, Res., i. § 1333.

§ Comptes Rendus, xxx. p. 440.

‖ Astr. Journ., i. p. 106.

chemical action of the current on the prepared paper at the signal-stations,—and while entertaining the most profound admiration for the remarkable ingenuity and care shown by Fizeau, I cannot consider his method as capable of giving results worthy of implicit reliance.

Walker's conclusions respecting the velocity are by no means inconsistent with Wheatstone's experiment; for the tension of the electricity and the conducting power and size of the wire differ so much in the two cases as to prohibit any comparison. Fizeau's, on the contrary, stand in the directest opposition to it, for he finds that neither the tension of the transmitted electricity, the intensity of the current, nor the size of the conductor, exerts any influence on the velocity; and at the same time makes the velocity through his copper wire to be 110,000 miles a second, while Wheatstone found 288,000 to be the minimum limit of the velocity through the copper wire which he used.

The remaining questions to which I propose to apply the results of the Washington and St. Louis experiment, are—

1st. Whether the stations on the line received the signal-pauses successively in their order of distance, and after intervals directly proportionate to their distance from the place where the signal was made?

2d. If the first question be decided in the affirmative,—by what route the signals reached the several stations; whether they were communicated through the earth, as Mr. Walker believes, when the distance between the stations is less by the earth's surface than through the wires, and if so, with what velocity; or whether they uniformly traverse the wires, and if so, with what velocity?

3d. Whether the intensity of the current occasions any appreciable difference in the velocity,* or whether the interposition of a battery between two stations affects the velocity appreciably?

In all the measurements for the determination of velocity, I have used the electrotomes only, and the results are thus entirely independent of the pass-time of the main circuit.

The Pittsburgh and St. Louis registers are both very indistinct, owing partly to want of distinctness in the impression of the graving tool, and partly to the bad quality of the paper of which the fillet is made. The Pittsburgh register is in many places illegible owing to the shortness of the pass. This could be spared with less inconvenience than any one of the others, but it is a very unfortunate circumstance that the St.

* Fechner, Repert. ii. 407. Poggendorf, Annalen, lii p. 497. De la Rive, Archives de l'Electricité, i. 533.

Louis record, the most important of all, should be so indistinct. It is affected, moreover, with the additional disadvantage, that the motion of the fillet was slow, and the liability to error in measuring thus greatly increased.

The labor of measuring off the intervals on the fillet is great and very tedious, and the average error of reading amounts to some hundredths of a second, although the intervals were mostly measured with dividers and a metallic scale, not with a diagonal scale of horn.*

All the results adduced depend upon the mean of a great number of readings, more than 5000 measurments of the work of February 4 having been made, in order to obtain them. The probable error has been computed in each case, and indicates the extent to which they are reliable. The corresponding minutes are recognized without difficulty by the pencilings made on the fillet at the time by the operators; but the part of the fillet corresponding to the time of winding up the weight which moved the registering apparatus was not always noted,—an omission which it may readily be supposed has occasioned much annoyance.

To estimate the length of wire between the several stations, ten per cent. has been added to the distances given in the books of reference. This is, I fear, too small an allowance for the sagging and zigzag course of the wires, but may be used in the absence of positive knowledge on the subject, and gives the following distances for the several stations as measured on the wire.

Washington,				
288	Pittsburgh,			
622	334	Cincinnati,		
747	459	125	Louisville,	
1045	757	423	289	St. Louis.

The experiments occupied a period of several hours, the Seaton clock graduating the scale, at Seaton-station, Washington City, Pittsburgh, Louisville, and St. Louis. The operator at St. Louis gave arbitrary signals by breaking circuit from time to time, at intervals of two or three seconds, which signals were recorded at all the stations. This he did for two successive minutes, twice during the evening. The same was done by the operators at Louisville, Cincinnati, and Pitts-

* Proc. Amer. Assoc., 1849, p. 189.

burgh. Signals were also made at the latter station for ten minutes after the Seaton battery was removed from the part of the circuit between the clock and the ground, and interposed between the clock and Pittsburgh.

The number of measurements used to obtain the mean difference of the registers is so large, that the addition or subtraction of fifty consecutive measurements exerts no appreciable influence on the result.

We have seen that when the clock-pauses are made at Washington, and the arbitrary signal-pauses at St. Louis, the difference of the interval between the pauses on the registers of the stations corresponds to the time occupied by the current in traversing *twice* the distance between the stations,—the clock-pauses being recorded later, and the signal-pauses earlier at the signal-station, by the same amount. The same is true of intermediate places, the excess of the recorded interval at the clock-station being greater by an amount equal to twice the distance between the stations whose fillets are compared, divided by the number of miles traversed by the voltaic current in a second.

Let us now consider the effect of a signal not made at a terminal station, but at some intermediate one. For all places beyond this, the two pauses occur later than the corresponding one at the signal-station, by the same interval. So that the decrease of the recorded interval on the registers of the successive stations measures the velocity for twice the distance from Washington until we reach the station where the signal was given. But for all places beyond this signal-station, the interval remains the same, and its record should be identical with that of the signal-station itself. We are thus enabled to control our estimate of the velocity between the clock and the signal-station, by the records made at all the stations beyond.

I find the following to be the mean excesses of the intervals between the signal-pauses and the preceding clock-pauses on the Washington fillet, over those on the fillets at the other stations.

WASHINGTON EXCESSES.

Register.	Signals.			
	P.	C.	L.	St. L.
Pittsburgh, ...	0·0295	0·0283	0·0373	0·0451
Cincinnati, ...	·0317	·0752	·0843	·0950
Louisville, ...	·0347	·0750	·1163	·1343
St. Louis, ...	·0455	·0704	·1108	·1451

Confining ourselves at first to the records of the signals made at St. Louis, we find the several registers to indicate the following velocities:

Pittsburgh, ...	12772	miles in a second.
Cincinnati, ...	13095	"
Louisville, ...	11124	"
St. Louis, ...	14404	"
Mean, ...	12851	"

The agreement of these numbers I consider very satisfactory. It will be observed that the velocity indicated by the St. Louis register is much greater than that derived from the others. Now the distance from Washington to St. Louis is 1046 miles by the wires, but only 742 in a geodetic line; and the question occurs very naturally whether this apparently greater velocity may not be a consequence of the transmission of the signal by the shorter part of the circuit through the ground, rather than along the wires. The distance to Louisville by the wires is 747 miles, and if the signals are transmitted through the ground to St. Louis with the same velocity as through the wire to Louisville, the time of transmission would be the same for both places. The results of measurement in these experiments show that a longer time was required for the signals to reach St. Louis than Louisville; though the greater length of time does not appear to have been proportionate to the greater distance. Those who believe the signals to be transmitted through the earth (when this distance is shorter), explain both these circumstances by assuming what is very probable, that the velocity in the ground is different from that in the wire. The data of our experiment would furnish everything necessary to decide this question, did the measurements accord sufficiently well with one another. But when it is remembered that we are dealing with such quantities as hundredths and thousandths of a second, measured too by dividers and a metallic scale, on paper of different qualities, whose hygrometric contractions and expansions are not only very great, but unequal,—we cannot expect any very close accordance, in the results of different measurements.

Giving the other data more in detail, and determining the probable errors of the means by the method of least squares, we have,

ST. LOUIS SIGNALS.

Register.	No. obs.	Interval.	Prob. error.	Velocity.	Min. limit.	Max. limit.
P.	37	0s·0373	0s·00267	15442	14410	16633
C.	46	0 ·0844	0 ·00148	14748	14494	15012
L.	46	0 ·1163	0 ·00149	12846	12684	13012
St. L.	36	0 ·1108	0 ·00173	13484	13276	13698

LOUISVILLE SIGNALS.

Register.	No. obs.	Interval.	Prob. error.	Velocity.	Min. limit.	Max. limit.
P.	56	0·0451	0·00252	12772	12096	13527
C.	60	0·0950	0·00302	13095	12691	13525
L.	65	0·1343	0·00252	11124	10919	11337
St. L.	61	0·1451	0·00329	14404	14085	14738

CINCINNATI SIGNALS.

Register.	No. obs.	Interval.	Prob. error.	Velocity.	Min. limit.	Max. limit.
P.	32	0·0283	0·00459	20400	17513	24294
C.	32	0·0752	0·00486	16543	15534	17685
L.	32	0·0750	0·00489	16587	15571	17744
St. L.	26	0·0704	0·00442	17670	16627	18854

PITTSBURGH SIGNALS.

Register.	No. obs.	Interval.	Prob. error.	Velocity.	Min. limit.	Max. lim.
P.	44	0·0295	0·00154	19525	18557	20601
C.	36	0·0317	0·00233	18170	16927	19612
L.	37	0·0347	0·00258	16591	15451	17922
St. L.	10	0·0405	0·00249	12659	12003	13392

Classifying these according to the stations whose distances are measured by the recorded values.

EXPERIMENTS OF FEB. 4, 1850.

Stations.	Mean Interval.	No. obs.	Ded'd. Velocity.
Pittsburg, ...	0·03567	152	16147 miles per second.
Cincinnati, . . .	0·08289	196	15008
Louisville, . . .	0·12291	147	12155
St. Louis, ...	0·14510	61	14404

The combination of all these, according to the method of least squares, gives, as the result of the experiments of February 4, a velocity of 14,900 miles, with a probable error of the mean $= \pm 10$.

These last tables seem to answer conclusively the first of the questions just propounded,—and at the same time to suggest the second.

We are justified in assuming that the signals, given by making and breaking the galvanic circuit of the telegraph, reach the several stations successively in their order of distance, and travelling with a finite and measurable velocity. But do they reach the terminal station *through the wires* first, when the distance through the ground is shorter?

Were the arrangement of the telegraph such now as it was at first, one source of uncertainty would perhaps have been avoided in the experiments; but the opportunity of solving this latter problem would have been lost. Until telegraphers availed themselves of the discovery of Steinheil, that no control over the circuit was lost when one half of it was formed by the earth, each telegraph line was double—consisting of one wire to the terminus and another back. But in all the lines in use in this country, the earth forms one half of the circuit. Are we to consider, when the two distant extremities of a line of wire communicate with the earth at a distance of many hundred miles from one another, that there is a special line of tension through the earth from one extremity to the other? and that a signal is communicated from terminus to terminus through the ground, in the same manner as it is through the wire? or may we consider the earth as a huge receptacle, to speak metaphorically, capable of receiving or imparting any amount of electricity at any time? The former opinion is held by my friend Mr. Walker.

But does it not seem improbable that the slight activity of a galvanic battery, traversing a circuit of 1000 miles of wire, should be sufficient to establish a *special* line of electric tension extending through the earth in a cord or parallel with the surface for 750 miles? For my own part, when I remember not only the grand phenomena of terrestrial magnetism,* but the immense galvanic force which must† be exerted by the mutual influence of the huge masses of metal in the bowels of the earth,—when I consider the mighty electrical activity developed‡ in the great processes of nature,—I will confess that I cannot bring myself to believe that *one special continuous line* of electric tension in the ground between two remote stations can be established athwart all these colossal forces by the action of a puny telegraph battery.§ Still,

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* Faraday, Researches, ii. p. 151.

† Phil. Trans., 1830.

‡ Pouillet, Ann. de Chim. et de Phys., xxxv. p. 414; Becquerel, Traité, iv. pp. 164, 188; Bibliothéque Univ., xiv. pp. 129, 171; Faraday, Researches, ii. pp. 47, 93.

§ I say *special* line of tension, meaning by this a line, whether independent or resultant, which is capable of conveying electrotome and electropœa signals in the

any views must be presented with diffidence, which vary from the expressed opinions of some of the greatest of our scientists.

The view which I take appears to be corroborated by the Coast Survey experiments of Feb. 4, in a two-fold manner.

First, if we suppose the clock-signals, starting simultaneously from the Seaton Station in two directions, to be propagated both through the wires and through the earth to the terminus at St. Louis, we must assume one of two things:—

1. That the signal which traverses the ground, moves with a velocity bearing precisely the same ratio to that through the wire, that the distance by one route bears to the distance by the other. This would be almost infinitely improbable, had we only the St. Louis experiment to guide us. But we have the results of the Coast Survey experiments to Cincinnati and to Charleston, which prove this hypothesis to be incorrect.

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same sense that a wire conveys them. After the admirable analytical investigations* of Smaasen, and the corroborative though independent researches† of Ridolfi, the character of conduction through the earth can no longer be doubtful. According to Baumgartner's experiments,‡ the earth's resistance to conduction of the voltaic current would appear dependent in a great measure upon the geological formation of the localities traversed, as his attempts to measure the earth's conductive power gave very different numerical results in different directions from Vienna. Baumgartner infers from his experiments that the curved lines traversed by the current between the electrodes do not diverge so much as the researches of Smaasen and Kirchoff § would lead us to suppose. But are not his experiments exposed to the criticism, suggested‖ by Smaasen, that we must know the precise polarization of the plates before we are warranted in forming any inference as to the earth's resistance? Any attempts to infer the conductive power of the earth from the Coast Survey experiments of 1849, Jan. 23, or Oct. 31, or of 1850, Feb. 4, Feb. 5, or July 8, by assuming that the earth conducted telegraphic signals, would assign to the earth a smaller conductive power than that of iron wire,—a result in the most direct contradiction to the experiments of Jacobi,¶ Matteucci,** and Baumgartner.

But if we assume that the earth can, under the most favorable circumstances, conduct signals as a wire does, this assumption vitiates the experiments of Mitchel,†† and of Fizeau and Gounelle.‡‡

* Pogg. Annalen, lxix. p. 162, et seq.; lxxii. p. 435.

† Il Cimento, 1847, May, June. v. Pogg. Annalen, lxii. p. 449.

‡ Sitzungsberichte d. Wiener Acad., 1849, May 10, July 5.

§ Pogg. Annalen, lxxii. p. 497; lxxv. p. 189.

‖ Pogg., Annalen, lxxii. p. 448.

¶ Pogg., Ann., lxviii. p. 488.

** De la Rive, Archives de l'Electr., xvii. 157.

†† Astr. Jour., i. p. 43.

‡‡ Comptes Rendus, xxx. p. 437.

2. We may suppose the velocity through the earth to be so small, that the electrotome, after a passage of 742 miles through the earth, has not reached St. Louis until the whole signal-pause and electropœa have been transmitted through the wire 1049 miles. On this theory, the passage of the 742 miles through the ground would occupy more than a quarter of a second, and we cannot tell how much longer time.

The second argument, in support of my view, is also derived from experiment, and although not consisting of so direct a negation as the first, partakes of the nature of a *reductio ad absurdum* in a sufficient degree to be perhaps yet more convincing than the former. It depends on the comparison of the records of signals made at St. Louis and at the nearer stations.

If we assume that the signals between Washington and St. Louis were transmitted through the earth, it is easy, by the comparison of the Louisville and St. Louis fillets, to determine the velocity of propagation in the ground. The St. Louis register gives, on the assumption above made, a velocity through the earth which would make the time of transmission for 742 miles between the termini equal to that required for traversing 528 miles of the wire. This amounts to the same thing as if the current between the termini traveled with the same velocity as in the wire, but through a distance of only 528 miles. For convenience of expression, I will speak as though this were the case. The result would be precisely the same.

We have, then, on comparing the different registers, two kinds of cases—one in which the distance between the stations is shorter through the wire, and the other in which it is shorter through the ground. We deduce the velocity for the latter instances on each hypothesis—and then inquire which best accords with the velocity computed from those cases where both hypotheses would give the same result?

The following table furnishes the materials for this comparison. The first hypothesis is, that the signals traversed the wire alone; the second, that they traversed the shorter route through the ground. In one column are the relative distances on each hypothesis;—in the other the deduced velocities. I infer from the discrepancies in the deduced velocities, that the improbability of the second hypothesis is exceedingly great.

HYPOTHESES COMPARED.

Signal.	Register.	Relative Distance.		Corresponding Velocity.	
		Hyp. I.	Hyp. II.	Hyp. I.	Hyp. II.
St. L.	P.	576	59	12800	1311
P.	St. L.	576	59	10473	1073
St. L.	C.	1244	727	13097	7651
C.	St. L.	1244	727	17771	10386
St. L.	L.	1494	977	11405	7458
L.	St. L.	1494	977	13484	8817
St. L.	St. L.	2090	1055	14415	7283

All the results of the experiments made by the Coast Survey for the determination of the velocity of the galvanic current, have been most kindly placed at my disposal by my friend Mr. Walker, to whose immediate direction the entire work had been confided by the superintendent of the Survey, our honored President. These furnish the materials for a series of tables, containing the deduction from all the experiments. Collecting in one table all the cases where the current must have passed through the wires, and in another all those instances where the ground furnished part of the shortest circuit, we have two equations with two unknown quantities, viz., the velocity in the ground and in the wires. Determining the latter independently, we find the velocity deduced from all those cases where the distance is shorter through the wire, to be, by twenty-six different comparisons, dependent on 768 readings, 15,600 miles a second. Substituting this value in the other equation, we obtain the number of miles of wire to which the time of transmission corresponded. These equations may be formed by aid of the method of least squares. The discussion of the results of all the experiments of the Coast Survey, namely, those on the lines from Washington to Cambridge, Washington to Cincinnati, Washington to Charleston, and Boston to New-York, show that the assumption of a transmission through the ground, with a constant velocity, does not materially improve the accordance of the observations. Indeed, in the experiments of February 4, the only indication of a transmission through the ground is to be found in the fact, that the velocity derived from the St. Louis observations can be made to accord better with the mean of the other values, by assuming that the signals traveled 1030 miles instead of 1045. The probable error of our estimate of the length of the wire is much greater than this.

The cases depending on 920 measurements, in which the shortest route is through the ground, are twenty-two in number. The velocity deduced, on the assumption that the signals traversed the ground, would be 11,200 miles in a second.

From all these considerations, I infer that in the St. Louis and Washington experiments, which were, of all that have been made, the most favorable for exhibiting the phenomena, the signals were in no case transmitted through the ground.

We have thus endeavored to take account, as far as possible, of all the sources of error which cannot be avoided, and to escape all those which can be. Our results, obtained from different data, accord so well with one another, as to make it appear very improbable that the velocity of propagation of the electric state produced in the iron telegraph wires by a galvanic battery is more than 20,000 or less than 12,000 miles per second.

From the combination of all the Coast Survey experiments with the electro-magnetic telegraph I have endeavored to deduce a measure of the velocity which shall be as reliable as the nature of the case permits. The difference of the temperature, at stations so distant from one another, makes it appear unadvisable to introduce any correction for temperature, even were such a refinement of the same order of magnitude with the unavoidable errors of our measurements, and therefore congruous with their character. The temperature was, in all cases, lower than the freezing point—the insulation not being sufficiently perfect at other times to allow communication between very distant stations. The temperatures reported were as follows :—Washington, + 8° F.; Pittsburgh, 0° ; Cincinnati, + 3° ; Louisville + 3° ; St. Louis + 3°.

The distances used are the following, in miles—ten per cent, having been added, as before :

Washington,			
172 Philadelphia,			
381	**209** New-York,		
850	**469**	**260** Cambridge,	

Washington,			
74 Harper's Ferry,			
165	**91** Cumberland,		
309	**235**	**144** Wheeling,	
578	**504**	**413**	**269** Cincinnati.

Washington to Cambridge, through the ground,				**380**
Washington to Cincinnati,	"	"	"	**403**

The great length of wire assigned to the part between New York and Philadelphia is a consequence of the distance to which the line ascends the Hudson before finding a crossing-place.

Date.	Terminus.	Velocity.	Prob. error.
1849 Jan. 23,	Cambridge,	18000	150
" Oct. 31,	Cincinnati,	18330	124
1850 Feb. 4,	St. Louis,	14900	10
" Feb. 5,	Charleston,	16856	

The resultant is 15,890 *miles per second, as the most probable value.*

Mr. Walker's experiment, 1850, July 8, with the Electro-chemical Telegraph,* gave for the velocity between Boston and New York (220 miles) 13,333 miles a second. The wire of Bain's Telegraph is, however, of a different size from the other lines, and is coated over, to prevent oxydation. Should we assume the signals to have been transmitted through the ground, Feb. 5, and July 8, we should obtain the respective velocities 10,690 and 10,820 miles per second.

In spite of the mutual confirmation of the several results by one another, and although the accordance is even remarkable, considering the numerous obstacles and sources of error, it is nevertheless true that the observations might be still better represented by supposing the velocity to be different at different parts of the line. Is this assumption admissible? It is, if we have reason to suppose that the velocity is dependent on the intensity of the current—for several batteries were interposed at intermediate stations on the line. The most powerful battery was at Pittsburgh, consisting of fifty Grove's cups, each holding a pint. The comparison of the different velocities appears to indicate that the greatest velocity of propagation was between Pittsburgh and Washington, and Pittsburgh and Cincinnati; but I am not disposed to lay stress on this point. In order to determine whether the velocity of the current was the same, when a battery was interposed between the signal and registering station, Mr. Walker made, as has been said, a special experiment, by continuing the Pittsburgh signals for ten minutes after the Washington battery had been changed from between the ground connection and the clock, to part of the circuit between the clock and Pittsburgh. The difference between the times of transmission given in the two cases is so small as to be practically inappreciable, and probably owing to the unavoidable errors of reading.

Eight measurements on the Pittsburgh and Cincinnati registers, where the battery was not interposed, gave as the time of transmission from Washington to Pittsburgh, $0^s.03049$. Seventy-four measurements on the same registers when the battery was interposed gave this time of transmission $0^s\,03106$.

Yet this does not furnish an answer to the question, whether the velocity is a function of the intensity of the current; for this intensity can hardly be varied by a transfer of either the circuit-breaker or the recording apparatus to the other side of the battery.

I have taken no account of time requisite for the development of the

* Ast. Jour. i., p. 105.

galvanic current,—inasmuch as, according to the experiments of Jacobi,* this time must certainly be a quantity of the second order, when compared with the time requisite for the propagation of the electric force along 1,000 milesof iron wire.†

There are several cases in which the line separating the clock-pause from the signal-pause precedes the clock-pause on the St. Louis register and follows the corresponding one on the Washington registers. This was first discovered by Mr. Walker, who considers‡ it to indicate that waves caused by breaking and closing the circuit may travel in opposite directions and cross one another, without interference. This view is theoretically startling to those who have looked on electric phenomena as exhibitions of a polar force, and I have examined with great care all the cases of this kind which could be found. In none of them can I find the distance between the signal-pauses on the two fillets to correspond, even approximately, to the velocity which the comparison of the same fillets indicates. The interval is generally twice as great as would be due to the time of transmission. I have been compelled to look in a very different direction for the explanation of the phenomenon. The St. Louis operator often struck the break-circuit key twice in quick succession; and in the cases under discussion it would appear that the interval between these two consecutive signal-pauses was very nearly equal to the time of transmission between St. Louis and Washington, and that one of the signal-pauses was confounded with the clock-pause at St. Louis, and the other with the clock-pause at Washington.

Two circumstances tend to corroborate this view:

1. The length of the clock-pause is in all these cases greater than the average; and

2. The interval between the clock and signal-pauses is neither too great nor too small to correspond with this hypothesis.

There are several other points which I would gladly have discussed; I will, however, confine myself to the mention of a single curious fact. Reducing the velocity found by Wheatstone for friction-electricity in copper, by multiplying it with the ratio of the conductive power of the two metals, we have 51,096 as the corresponding velocity in iron wire of the same diameter (1.75 millimetres), at a temperature

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* Pogg. Ann., xlv. 23.

† Haldat, Comptes Rendus, vi. 52. De la Rive, Arch. de l'Electr., iii. 288.

‡ Proceedings Amer. Assoc., Charleston, 1850, p. 124.

of 32° F. This is, to the velocity we have deduced, almost precisely in the inverse ratio of the sectional areas of the conducting wires.

It is to be hoped that still further materials may be collected during the ensuing winter, by the zeal of the Superintendent of the Survey and the indefatigable energy of Mr. Walker. The superior insulating power of the posts in freezing weather renders winter the most desirable season for instituting experiments of this kind. It is a circumstance in which we, as Americans, may be warranted in feeling an honest pride that all the telegraph experiments, excepting those of Mitchel and Fizeau, which have been instituted for deducing the velocity of the galvanic current—a result so important to science, and for the proper discussion of telegraph observations for longitude—have been made under the superintendence of the U. S. Coast Survey.

In future experiments, it appears to me highly desirable that some machinery capable of delivering several inches of paper per second, at a tolerably uniform rate, should be employed to move the fillet or cylinder on which the record is made; that the quality of the paper used should be as fine as possible; and that the adjustments of the pass and spiral spring should remain unchanged during the continuance of the experiment. Further experiments with Bain's Chemical Telegraph are desirable, if means can be devised for recording at each extremity of the line by one and the same circuit, which is not improbable.

The ordinary size of the telegraph wires is, as I understand, that corresponding to a weight of about 300 lbs. to the mile. The new line now constructing, by the patentees of House's wonderful printing telegraph, between Buffalo and New York, is composed of wires weighing from 600 to 800 lbs. to the mile. Experiments made with this line would be of great service in determining whether the velocity varies with the section of the conducting wire.

THIRD DAY, AUGUST 21, 1850.

GENERAL MEETING.

(Evening Session.)

On the Volcanoes of Central America, and the Geographical and Topographical Features of Nicaragua, as connected with the proposed Inter-Oceanic Canal. By E. G. Squier, Esq., Chargé d'Affaires of the United States to the Republics of Central America.

No equal extent of the American continent, perhaps of the globe, possesses so many volcanoes, active and extinct, or exhibits so many traces of volcanic action, as Central America; that is to say, the region embraced between the Isthmus of Tehuantepec and that of Panama, or Darien. In the words of an eminent traveler, the entire Pacific coast of this remarkable country "bristles with volcanic cones," which form a conspicuous feature in every landscape, rising above the plains and undulating hills, and often from the edges of the great lakes, with the regularity and symmetry of the pyramids. It is a matter of surprise and regret that, affording as it does, so excellent a field for studying the grand and interesting phenomena connected with volcanoes and earthquakes, this country has not more particularly attracted the attention of scientific men, and especially of those who ascribe to igneous and volcanic agency so important a part in the physical changes which our planet has undergone. Humboldt did not pass through Central America, although fully impressed with the importance of its geological and topographical investigation; a deficiency which he deplores in many places in his published researches. Nor am I aware that any but very partial and imperfect accounts have been given to the world of the volcanoes of this country, and those have been by persons claiming no consideration as scientific men. Recognizing fully my own deficiency in this respect, I should not think of venturing on the subject, except in the hope of directing the attention of competent persons anew to it, and thus contributing to supply the desideratum. And I may here observe that the difficulties and dangers which are popularly supposed to beset travelers in Central America, resulting from an unsettled state of society, and from tropical diseases and imperfect means of communication, are fanciful, or have been recently in great part obviated. The student will find few serious obstacles to his investigations, and if he possess a just enthusiasm, without which he is no true student, he will find means to surmount them.

The time, too, is especially favorable to investigation on the part of our countrymen; for apart from the good feeling which pervades all classes toward us, down to the poorest Indian, the Governments of the various States are both willing and anxious to extend every possible facility to men of science who may visit the country, especially those versed in geology and mineralogy. Indeed, I have been both personally and officially urged to exert my influence to induce such to come there, and also authorized to assure them of a cordial welcome, and all practicable assistance on the part of the authorities.

The speedy opening of regular steam communication with Nicaragua, by means of which the interior of the country may be reached in ten days from New York, will, I am sure, also contribute towards inducing our men of science to explore this almost virgin field.

The volcanoes of Central America are all situated on the Pacific coast; the eastern slope of the continent consisting of broken mountain ranges, which exhibit few traces of volcanic action. In fact, they occur almost in a right line, running due N. W. and S. E., commencing with the high volcano of Cartago in Costa Rica (14,000 feet high), from the summit of which both oceans are visible, to Citlaltepetl, in the department of Vera Cruz, in Mexico. There are several hundred volcanic peaks and extinct craters on this line, the most remarkable of which are Cartago, Irasu, Turrialva, Barba, and Vatos, in Costa Rica; Abogado, Cerro Pelas, Miriballes, Tenerio, Rincon de la Vieja, Orosi, Madeira, Ometepec, Zapetero, Solentinami, Momobacho, Masaya, or Nindiri, Managua, Momotombo (6,500 feet high), Las Pilas, Acosusco, Orota, Telica, Santa Clara, El Viejo (7,000 feet high), Coseguina, and Joltepec, in Nicaragua; El Tigre, Guanacaure, and Nacaome, in Honduras; Amapala or Conchagua, San Salvador, San Miguel, San Vicente, Isalco, Paneon, and Santa Ana, in San Salvador; Pacaya, Volcan de Agua, Volcan de Fuego, Incontro, Acatenango, Atitlan, Tesanuelco, Sapotitlan, Amilpas, Quesaltenango, and Soconusco, in Guatemala. There are many others which are nameless, or of which the names are unknown. Some ten or twelve of those above named are said to be "alive" (vivo), that is to say, they throw out smoke, and exhibit other evidences of vitality. But three or four, however, can be said to be active at present, of which, Isalco, in San Salvador, is the most remarkable, having been formed within the last eighty years, and within the recollection of persons now living. This volcano, and that of Jorullo, in Mexico, described by Humboldt, are, I believe, the only ones which have originated on the continent since the discovery. It

arose from the plain in 1770, and covers what was then a fine cattle hacienda or estate. The occupants on this estate were alarmed by subterraneous noises and shocks of earthquakes, about the end of 1769, which continued to increase in loudness and strength until the 23d of the February following, when the earth opened about half-a-mile from the dwellings on the estate, sending out lava, accompanied by fire and smoke. The inhabitants fled; but the *vaqueros*, or herdsmen, who visited the estate daily, reported a constant increase in the smoke and flame, but that the ejection of lava was at times suspended, and vast quantities of ashes, cinders, and stones sent out instead, forming an increasing cone around the vent, or crater. This process was repeated for a long period, but for many years the volcano has thrown out no lava. It has, however, remained in a state of constant eruption, the explosions occurring every sixteen minutes and a quarter, with a noise like the discharge of a park of artillery, accompanied by a dense smoke and a cloud of ashes and stones, which fall upon every side, and add to the height of the cone. It is now about 1,500 or 2,000 feet in height, and I am informed by an intelligent West Indian gentleman, Dr. Drivon, who has known it for the past twenty-five years, that within that period it has increased about one-third. At some times the explosions are more violent than at others, and the ejected matter greater in amount; but it is said the discharges are always regular. With the wind in a favorable direction, an annoying and sometimes injurious quantity of fine ashes or powder is carried to the city of Sonsonate, twelve miles distant. The volcano of Jorullo rose, I believe, in a single night; but as we have seen, Isalco is the result of long continued deposits, and it seems to me that most of the volcanoes of Central America, including some of the largest, have been formed in like manner. In fact I have been a personal witness of the origin of a new volcano, which, if it does not meet a premature extinguishment, bids fair to add another high cone to those which now stud the great plain of Leon. This plain, the finest I have ever seen, lies between Lake Managua, (which has its outlet through Lake Nicaragua and the River San Juan into the Atlantic) and the Pacific. Here the Cordilleras are wholly interrupted, and the Almighty hand has smoothed the way for the grandest enterprise which human daring has conceived, and which human energy seems now on the eve of accomplishing,—the opening of a Ship Canal between the two oceans. Standing upon a moderately elevated point in this great plain, the traveler looks down upon one hand on the silvery waters of Managua, and upon the other on the broad Pacific, stretching far away to "Cathay, and the Spice Islands."

A narrow belt of land less than 60 feet higher than the Lake, alone prevents Managua from sending her waters in the Western rather than the Eastern Ocean—into the placid Pacific rather than the turbulent Atlantic. This plain is traversed by a succession of volcanic cones, from the gigantic Momotombo, standing boldly out into the Lake, to the memorable Coseguina, projecting its base not less boldly into the Ocean. Fourteen distinct volcanoes occur within one hundred miles, on this line, all of which are visible at the same time. They do not form a continuous range, but stand singly, the plain between them generally preserving its original level. They have not been "thrust up," as the volcano of Jorullo seemed to have been, elevating the strata around them; although it is not certain but the original volcanic force, being general in its action, raised up the whole plain to its present level. All these are surrounded by beds of lava, called by the natives *mal pais*, literally "bad country," extending in some cases for leagues in every direction. The lava current in places seems to have spread out in sheets, flowing elsewhere, however, in high and serpentine ridges, resembling Cyclopean walls, often capriciously enclosing spaces of arable ground, in which vegetation is luxuriant: these are called by the natives *corrales*, yards. Hot springs, and openings in the ground emitting hot air, smoke, and steam, called *infernales*, are common around the bases of these volcanoes. For large spaces the whole ground seems resting upon a boiling cauldron, and is encrusted with mineral deposits. Around some of these volcanoes, that is to say those having visible craters, are many smaller cones, of great regularity, composed of ashes, volcanic sand, and triturated stones, resembling septaria. They seldom support any but a few dwarf trees, and are covered with coarse grass. This grass when green gives them a beautiful emerald appearance. In the dry season this color is exchanged for yellow, which, after the annual burning, gives place to black. They constitute with their changes very singular and striking features in the Central American landscape. On the 11th and 12th days of April last, rumbling sounds, resembling thunder, were heard in the City of Leon, situated in the centre of the plain I have described. They seemed to proceed from the direction of the volcanoes, and were supposed to come from the great volcano of Momotombo, which often emits noises, and shows other symptoms of activity, besides sending out smoke. This volcano, however, on this occasion, exhibited no unusual indications. The sounds increased in loudness and frequency on the night of the 12th, and occasional tremors of the earth were felt as far as Leon. Early on the morning of

Sunday, the 13th, an orifice opened near the base of the long extinguished volcano of Las Pilas, about twenty miles distant from Leon. The throes of the earth at the time of the outburst were very severe in the vicinity, resembling, from the accounts of the natives, a series of concussions. The precise point where the opening was made might be said to be in the plain; it was, however, somewhat elevated by the lava which had ages before flowed down from the volcano, and it was through this bed of lava that the eruption took place. No people reside within some miles of the spot; consequently I am not well informed concerning the earlier phenomena exhibited by the new volcano. It seems, however, that the outburst was attended with much flame, and that, at first, quantities of melted matter were ejected irregularly in every direction. Indeed, this was clearly the case, as was shown upon my visit to the spot some days thereafter. For a wide distance around were scattered large flakes resembling freshly cast iron. This irregular discharge continued only for a few hours, and was followed by a current of lava, which flowed down the slope of the land toward the West, in the form of a high ridge, rising above the tops of the trees, and bearing down everything which opposed its progress. While this flow continued, which it did for the remainder of the day, the earth was quiet, excepting only a very slight tremor, which was not felt beyond a few miles. Upon the 14th, however, the lava stopped flowing, and an entirely new mode of action followed. A series of eruptions commenced, each lasting about three minutes, succeeded by a pause of equal duration. Each eruption was accompanied by concussions of the earth, too slight, however, to be felt at Leon, attended also by an outburst of flame a hundred feet or more in height. Showers of red hot stones were also ejected with each eruption to the height of several hundred feet. Most of these fell back in the mouth or crater, the rest falling outward, and gradually building up a cone around it. By the attrition of this process, the stones became more or less rounded, thus explaining a peculiarity in the volcanic stones already alluded to. These explosions continued uninterruptedly for seven days, and could be accurately observed from Leon in the night. Upon the morning of the 22d, accompanied by Dr. J. W. Livingston, U. S. Consul for Leon, I set out to visit the spot. No one had ventured near it, but we had no difficulty in persuading some *vaqueros*, from the village of Orota, to act as guides. We rode with difficulty over beds of lava, until within about a mile and a half of the place, proceeding thence on foot. In order to obtain a full view of the new volcano, we ascended a high, naked ridge of scoriæ, entirely

overlooking it. From this point it presented the appearance of an immense kettle, upturned, with a hole knocked in the bottom, forming the crater. From this, upon one side, ran off the lava stream, yet fervent with heat, and sending off its tremulous radiations. The eruptions had ceased that morning, but a volume of smoke was still emitted, which the strong northeast wind swept down in a trailing current along the tree-tops.

The cone was patched over with yellow, crystallized sulphur, deposited from the hot vapors passing up amongst the loose stones. The trees all around were stripped of their limbs, leaves, and bark, and resembled so many giant skeletons. Tempted by the quietude of the volcano, and anxious to inspect it more closely, in spite of the entreaties of our guides, we descended from our position, and going to the windward, scrambled over the intervening lava beds, through patches of thorny cactuses and agaves, towards the cone. On all sides we found the flakes of melted matter which had been thrown out on the first day of the eruption, and which had moulded themselves over whatever they fell upon. We had no difficulty in reaching the base of the cone, the wind driving off the smoke and vapors to the leeward. It was perhaps a hundred and fifty or two hundred feet high, by two hundred yards in diameter at the base, and of great regularity of outline. It was made up entirely of stones, more or less rounded, and of every size, from one pound up to five hundred. No sound was heard when we reached it, except a low, rumbling noise, accompanied by a very slight tremulous motion. Anxious to examine it more closely, and to test the truth of the popular assertion that any marked disturbance near the volcanic vents is sure to bring on an eruption, we prepared to ascend. Fearing we might find the stones too much heated near the summit, I prepared myself with two staffs, as supports, and to save my hands. The Doctor disdained such appliances, and started without them. The ascent was very laborious, the stones rolling away beneath our feet, and rattling down the sides. We, however, succeeded in almost reaching the summit, when Dr. Livingston, who was a little in advance, suddenly recoiled with an exclamation of pain, having all at once reached a layer of stones so hot as to blister his hands at the first touch. We paused for a moment, and I was looking to my footing, when I was startled by an exclamation of terror from my companion, who gave simultaneously an almost superhuman leap down the side. At the same instant a strange roar almost deafened me; there seemed to be a whirl of the atmosphere, and a sinking of the mass upon

which I was standing. Quick as thought I glanced upward; the heavens were black with stones, and a thousand lightnings flashed among them. All this was in an instant, and in the same instant I too was dashing down the side, reaching the bottom at the same moment with my companion, and just in time to escape the stones, which fell in rattling torrents where we had stood a moment before. I need not say that in spite of spiny cactuses, and rugged beds of lava, we were not long in putting a respectable and safe distance between us and the flaming object of our curiosity. The eruption lasted for nearly an hour, interspered with lulls, like long breathings. The noise was that of innumerable blast-furnaces in full operation, and the air was filled with projected and falling stones. The subsidence was almost as sudden as the outburst, and we waited several hours in vain for another eruption. Our guides assured us that another attempt to ascend, or any marked disturbance on the slope, or in the vicinity, would be followed by an eruption, but we did not care to try the experiment.

From that period until I left Central America, I am not aware that there occurred more than one eruption, and that on the occasion of the falling of the first considerable shower of rain, on, I think, the 27th of May last. The discharges from this vent, consisting wholly of stones, may have been and probably were peculiar, for the volcanoes themselves, and the cones surrounding them, generally seem to have been made up of such stones, interspersed through large quantities of ashes and scoriaceous sand, alternating with beds of lava.

Although believing that most of the volcanic cones have been formed in this manner, by gradual accumulations, yet the volcanoes which have shown the greatest energy are low and irregular, and devoid of anything remarkable in their appearance. Such is the Volcano of Coseguina in Nicaragua, the eruption of which, in 1835, was one of the most terrible on record.

On the morning of the 20th of January in that year, several loud explosions were heard for a radius of a hundred leagues around this volcano, followed by the rising of an inky black cloud above it, through which darted tongues of flame resembling lightning. This cloud gradually spread outward, obscuring the sun, and shedding over everything a yellow, sickly light, and at the same time depositing a fine sand, which rendered respiration difficult and painful. This continued for two days, the obscuration becoming more and more dense, the sand falling more thickly, and the explosions becoming louder and more frequent. On the third day, the explosions attained their maximum, and the

darkness became intense. Sand continued to fall, and the people deserted their houses, fearing the roofs might yield beneath the weight. This sand fell several inches deep at Leon, more than one hundred miles distant. It fell in Jamaica, Vera Cruz, and Santa Fe de Bogota, over an area of one thousand five hundred miles in diameter. The noise of the explosions was heard nearly as far, and the Superintendent of Belize, eight hundred miles distant, mustered his troops, under the impression that there was a naval action off the harbor. All Nature seemed overawed; the birds deserted the air, and the wild beasts their fastnesses, crouching, terror-stricken and harmless, in the dwellings of men. The people for a hundred leagues groped, dumb with horror, amidst the thick darkness, bearing crosses on their shoulders and stones on their heads, in penitential abasement and dismay. Many believed the day of doom had come, and crowded to the tottering churches, where, in the pauses of the explosions, the voices of the priests were heard in solemn invocation to Heaven. The brighest lights were invisible at the distance of a few feet; and, to heighten the terrors of the scene, occasional lightnings traversed the darkness, shedding a lurid glare over the earth. This continued for forty-three hours, and then gradually passed away.

For some leagues around the volcano, the sand and ashes had fallen to the depth of several feet. Of course the operations of the volcano could only be known by the results. A crater had been opened, several miles in circumference, from which had flowed vast quantities of lava into the sea on one hand, and the Gulf of Fonseca on the other. The verdant sides of the mountain were now rough, burned, and seamed, and covered with disrupted rocks and fields of lava. The quantity of matter ejected was incredible in amount. I am informed by the captain of a vessel which passed along the coast a few days thereafter, that the sea for fifty leagues was covered with floating masses of pumice, and that he sailed for a whole day through it, without being able to distinguish excepting here and there, an open space of water.

The appearance of this mountain is now desolate beyond description. Not a trace of life appears upon its parched sides. Here and there are openings emitting steam, small jets of smoke and sulphurous vapors, and in some places the ground is swampy from thermal springs. It is said that the discharge of ashes, sand, and lava was followed by a flow of water, and the story seems corroborated by the particular smoothness of some parts of the slope. The height of this mountain is not, I think, more than two thousand five hundred feet. The anniversary of the

cessation of this eruption is celebrated in the most solemn manner throughout all Central America.

The volcano of San Miguel, in San Salvador, is a regular truncated cone, rising eight thousand feet above the plain. It emits a small plume of smoke from its top, but its eruptions, which are frequent, take place near the base. The last of these occurred in 1848, when several vents were opened about two thousand feet above the plain on its eastern slope. It was attended with none of those appalling phenomena which were observed at the eruption of Coseguina, and lava only was ejected with a little ashes, from the great crater. The lava currents radiate, from this mountain for many leagues in every direction, interposing vast barriers to the traveler in approaching it.

The volcano of San Salvador, near the city of that name, is remarkable for the extraordinary size of its crater, which is estimated by Dr. Weems, American Consul in Guatemala, to be nine miles in circumference, sloping like an inverted cone to the depth of five thousand feet—almost large enough to receive the entire volume of Vesuvius. A small lake is visible at the bottom.

The volcanoes in the vicinity of the city of Guatemala are better known than any others in Central America, chiefly in connection with the earthquake of 1773, which caused the abandonment of the city of Old Guatemala. This earthquake, however, does not seem to have been as violent as many others which have happened since. And the accounts which have been published of the earth opening and swallowing entire houses, vomiting fire, etc., are, as observed by a late writer, incorrect and absurd. And in copying the monkish accounts of the catastrophe, modern authors may as well add "the other interesting particulars of devils being seen to ascend out of the earth where it yawned, to assist in pulling down the sacred edifices, and wooden images of the saints running away and beckoning the inhabitants to follow them, with other occurrences equally novel and surprising."

A number of very severe earthquakes have happened within the last few years. One occurred in Guatemala in 1830, nearly, if not quite, as severe as that of 1773. In February, 1831, and September, 1839, severe shocks were felt in San Salvador, and in 1841 in Costa Rica. The last nearly destroyed the city of Cartago, which had previously suffered a similar catastrophe. May, 1844, was distinguished throughout Nicaragua by a series of earthquakes occurring at regular intervals, over a period of several days. The city of Nicaragua suffered much, and the waters of the Lake were observed to rise and fall with the

Much might be said on the phenomena of earthquakes as they occur in this country. The shocks seem to be of two classes; the perpendicular, which are felt only in the vicinity of volcanoes, and the horizontal, which reach over wide tracts of country. The latter are very unequal; in some places being violent, and in others, nearer their assumed source, comparatively slight. The undulating movement seems to be only a modification of the horizontal or vibratory. Sometimes these motions are all combined, or rather succeed each other with great rapidity. Such was the case with the earthquake of the 27th of October last, which I experienced, and of which I can speak more authoritatively. It occurred at about one o'clock in the morning. I was aroused from sleep by a strong undulatory motion, which was sufficiently violent to move my bed several inches back and forth on the rough paved floor, and to throw down books and other articles which had been placed on my table. The tiles of the roof were also rattled together violently, and the beams and rafters creaked like the timbers of a deeply-laden vessel in a heavy sea. The people all rushed from their houses in the greatest alarm, and commenced praying in loud tones. The domestic animals seemed to share the general consternation; the horses struggled as if to loose themselves, and the dogs commenced a simultaneous barking. This undulatory motion lasted nearly a minute, steadily increasing in violence, until suddenly it changed into a rapid vibratory or horizontal motion, which rendered it difficult to stand upright. This lasted about thirty seconds, and was followed as suddenly by a vertical movement, or a series of shocks, such as one would experience in being rapidly let down a flight of steps; then declined in violence, but nevertheless seemed to stop abruptly. The whole lasted about two minutes, and can be compared to nothing except the rapid movement of a large and loaded railroad car over a bad track, in which there are undulations, horizontal irregularities, and breaks.

No considerable damage was done. Some old walls were thrown down, and in various places around the country I have observed that rocks were detached and portions of cliffs broken off by the shocks. The thick adobe walls of my house were cracked in several places from top to bottom. Many other buildings suffered in like manner. The motion which seemed to me most dangerous was that which I have described as *horizontal*, in which the earth seemed to slide away from beneath my feet.

The night was clear moonlight, and it was very still; not a breath of air seemed stirring. The orange trees in my court-yard, during the

continuance of the undulations, swayed regularly to and fro; but when the other movements followed, they had an unsteady or tremulous motion. The water in my well, which is very deep, seemed also much agitated. The direction of the undulations was from north to south, and they were felt throughout the entire State of Nicaragua, and in Honduras and San Salvador, and even perhaps beyond these limits.

I learned from old residents that, as compared with the others which have occurred within the last quarter of a century, this earthquake ranked as about seven, the maximum being ten.

All observers here concur in saying that, while earthquakes are common at all times of the year, they are much more numerous and violent at the entrance and close of the two seasons, the wet and the dry; that is, about the last of October and the first of November, and the last of April and the first of May. They are observed as particularly numerous and severe after the heavy rains, at the close of the wet season in October. It is also observed that a general quiet seems to prevail, for a period, both before and after their occurrence.

It is difficult to discover the connection between these different phenomena; but there seems to be a concurrence as to the facts here stated. It is certainly true that the only shocks which I have felt were in the periods indicated, and it is also certain that nearly all occur in the night. Perhaps, amidst the occupations and distractions of the day, the lesser ones pass unobserved.

There are many striking features in the topography of Central America, which seem entirely due to volcanic agency. Those which have more particularly attracted my attention, are what are popularly denominated extinct craters, now partially filled with water, forming lakes without outlets or apparent sources of supply, save the rains. Some of these occur on the mountain and hill ranges, and are surrounded by evidences of having been volcanic vents. But this is not always the case. I will take what is called the Lake of Masaya as an instance. This is not less than ten or twelve miles in circumference, and is not far from one thousand feet, perhaps more, below the general level of the country. The sides are sheer precipices of trachytic rocks, splintered and blistered, and exhibiting every indication of having been exposed to the intensest heat. Yet, if these were true craters, where are the lava, ashes, and other materials which they have ejected? There are certainly none in their vicinity which have emanated from them, no traces of lava streams surrounding them, nor are their edges elevated above the general level. Upon one side of the particular one

which I have mentioned, rises the extinct volcano of Masaya or Nindiri, with its proper crater, whence have flowed vast quantities of lava, part of which, falling over the precipitous walls of the lake, have quite filled it upon that side. Some of the lakes are more or less impregnated with saline materials, but others are perfectly fresh, and abound in fish. The burned and blistered walls indicate, it appears to me, that they have not been caused by subsidence, or the falling in of the earth or rock strata.

The great plain of Leon, at its highest part, is elevated about two hundred feet above the sea; yet, in the vicinity of the range of volcanoes which traverse it, beds of lava, fifteen feet thick, have been found in digging wells, at the depth of seventy-five Spanish varas, or about two hundred and ten feet, and this at a point not the highest of the plain, but, according to my calculations, only one hundred and thirty feet above the ocean. Unless there is some great error in these data, and I can conceive of none, they would seem to prove that there has been a subsidence of the plain since the almost infinitely remote period when the bed of lava flowed upward from the depths of the earth. I may mention that in the vicinity of the volcanoes, water is scarce, and can only be obtained by digging to great depths. The particular well which I refer to is at a cattle estate, eighteen miles north-east of Leon, and is upwards of three hundred feet in depth, the water pure, with no saline materials in solution.

Mr. Squier here observed that he should omit any further references to volcanoes, although there remained many facts of interest to be presented, in order to submit, in compliance with request, some brief observations on the geographical and topographical features of that part of Central America embraced in the State of Nicaragua, particularly as connected with the proposed great inter-oceanic canal. He, however, wished to say, once for all, that he had no prejudices in favor of one or the other proposed routes of communication across the continent, his only wish being that the *best* might be decided upon, and the grand enterprise carried out. He did not appear as the advocate of one line of transit at the expense of the other, nor as the mouth-piece of any set of speculators. The subject of artificial communication between the oceans had, he said, attracted attention for more than three hundred years, and upwards of one hundred books and memoirs had been written on the subject—most, however, based upon exceedingly imperfect data, collected from various and often conflicting and unreliable sources. The great problem connected with the subject, and that which all have

esteemed as vital to the question of the communication, has been this: *Is the great chain of the Cordilleras interrupted at any point upon the great Central Isthmus?* This question, observed Mr. S., I am enabled to answer in the affirmative. Between the great Basin of Nicaragua, in which are Lakes Managua and Nicaragua, and which is drained by the River San Juan, flowing into the Carribean Sea—that is to say, between the western extremity of Lake Managua and the Pacific, the Cordilleras are *wholly interrupted*, and we have only the great plains of Leon and Canejo, rising, for a distance of three thousand yards, to an elevation of about sixty feet above the lake, and two hundred above the sea, and thence subsiding, in a gentle slope, to the ocean. It is through the Lake of Managua, and across this plain, either to the little port of Tamarinda or the fine port of Realejo, or else (and this is most probable) across the Llano del Conejo, into the magnificent Gulf of Fonseca, that the proposed canal must pass. It is true Lake Nicaragua approaches, at one or two points, to within about fifteen miles of the Pacific, and it would seem that it might be easily connected with that ocean. But between the two is a ridge of land of considerable height, across which no canal can ever be constructed, simply because no sufficient amount of water is to be obtained at or near the summit to supply even the *leakage* of such a work. And a direct cut by which a supply might be obtained from the lake, would involve a tunnel, large enough for ships, of some miles in length, besides immense and almost impossible cuttings for the remainder of the distance. This is sufficiently demonstrated by the survey, or rather reconnoisance, of Mr. Bailey.

It is said that a practicable route exists between Lake Nicaragua, by way of the river Sapoa, to the Bay of Salinas, a part of the Gulf of Papagayo. But no authentic examination has ever been made of this line, and Mr. Squier expressed his doubts as to its practicability. He could not, however, presume to speak decisively, as he had not enjoyed an opportunity of examining it in person. He would, nevertheless, say that, even if practicable, the contending winds which prevail in this Gulf, and to which it has given the name of Papagayos, would prove a signal disadvantage to it. Besides, so far as communication with California and the northern half of the continent, as well as with the Sandwich Islands and China and the East Indies is concerned, entering the ocean at this point would be eminently disadvantageous.

Mr. Squier continued by saying that a ship communication across the continent at this point involved, in his estimation, not only a canal from the great Nicaraguan Basin to the Pacific, but also a canal for the

greater part, if not the whole of the distance from Lake Nicaragua to the Atlantic. The River San Juan, the outlet of this Lake, although probably passing an equal if not greater amount of water with the Connecticut or Hudson rivers,—and although, for, by far the greater part of its length, capable of being navigated by our largest river steamers, is yet, for reasons which he had presented in a recent communication, in the *Providence Journal*, inadequate, under any practical improvement, of being made navigable for ships,—and any canal not admitting of the easy and rapid passing of the largest ships, would very imperfectly meet or subserve the great ends of its construction and of commerce. Small steamers, such as are used on the tributaries of the Mississippi, may now be run on the river; and with some improvements at the various rapids, steamers of a larger class may be employed.

It will, therefore, be necessary to cut a canal parallel to the river, from which the requisite supply of water may easily be drawn. The line would run upon the north bank of the stream, near the base of the hills which bound the valley on that side. It is possible the river might, by dredging, be used for a distance of twenty miles downward from the Lake to the River Savalos, in which case this section of the canal would be shortened to seventy, perhaps to sixty miles. Mr. Squier said that this part of the proposed line had hitherto been passed over with very little remark, on the assumption that here no difficulty existed, and that the river might readily be made to answer every desirable purpose; yet, in his estimation, it was by far the most difficult part of the whole enterprise; not on the score of feasibility, but of labor and expense. The whole matter, however, resolved itself into a simple question of dollars and cents. It only required capital.

The great Lake of Nicaragua, one hundred and twenty-five miles in length by fifty in breadth, although quite shallow at many points on the northern shore, is nevertheless of great general depth, varying from six to sixty fathoms, and may, without doubt, be made navigable for ships of the greatest draught. The only difficulties exist in approaching the shores at either extremity; but these may probably be obviated by pier work and dredging.

Between Lake Nicaragua and the superior lake of Managua is a nominal distance of eighteen miles. But the connecting stream, called Rio Tipitapa, is, in fact, for an extent of twelve miles, an *estuary* of Lake Nicaragua, and for that distance may probably be so cleared out as to be made navigable. From the head of this estuary, at a point called

San Pasquiel, to Lake Managua, a distance of five miles, there is a rise of about twenty-five feet.

The greater part of this rise is abrupt, at a point about a mile below the Lake, called the Falls of Tipitapa. Very little water passes here except in the rainy season, and in the dry season the stream is sometimes wholly suspended. The rock is apparently a soft calcareous breccia, easily worked. A short section of canal is therefore indispensable here, extending from the Lake to the Falls, a distance of one mile. Below this to San Pasquiel, at the head of the estuary from Lake Nicaragua, the bed of the stream is deep, and the banks abrupt and high, forming a natural canal, which only needs proper dams and locks at its lower extremity, to furnish a channel adequate to every purpose of navigation.

Lake Managua is a fine body of water, and of much larger size than has hitherto been represented. It is certainly not far from fifty or sixty miles in greatest length by thirty-five in width, and ranges from two, to ten and fifteen, and even forty fathoms in depth. The scenery which borders it is unsurpassed in beauty and grandeur. Upon the northern and eastern shore, lifting their blue rugged peaks one above the other, are the mountains of Matagalpa, merging into those of Segovia, rich in metallic veins. Upon the south and west are broad and fertile slopes and level plains, covered with luxuriant verdure, and of almost unlimited productiveness. The volcano of Momotombo, like a giant warder, stands out boldly into the lake, its bare and blackened summit, which no man has ever reached, covered with a light wreath of smoke, attesting the continued existence of those internal fires which have seamed its steep sides with burning floods, and which still send forth hot and sulphurous springs at its base. Within the lake itself rises the regular cone of Momotombita, so regular that it seems a work of art, covered with a dense forest, under the shadows and within the deep recesses of which, frayed by the storms of ages, stand the rude and frowning statues of the gods of aboriginal superstition, raised long before European feet trod the soil of America, and to which the mind of the Christianized Indian still reverts with a mysterious reverence impossible to conceal.

Between the north-western extremity of this lake and the Pacific, is the great plain already mentioned. Three lines across this plain have been suggested; 1st, by the left shore of the lake to the small port of the Tamarinda; 2d, by the same shore to the well known port of

Realejo; and 3d, by the upper shore of the lake to the Gulf of Fonseca or Conchagua. It is probable that all of these lines are feasible, but a minute survey can alone determine which is best.

The first line suggested, that to the port of the Tamarinda, is considerably shorter than either of the others,—not exceeding fifteen or eighteen miles in length. But, said Mr. Squier, the water of the lake upon its north-western shore is shallow. He had sounded it in July, 1849. It deepened regularly from the shore to the distance of one mile, when it attained five fathoms. After that it deepened rapidly to ten and fifteen fathoms. The country between the lake and the Tamarinda, so far as can be ascertained, it being covered with forests, is level, and offers no insuperable obstacle to a canal. There is no town or village near the port, and it seems to have escaped general notice. Nor is it known that it has ever been entered by vessels, except in one or two instances, for the purpose of loading Brazil wood. It is small, but tolerably well protected. It is not, however, a proper termination for a work like the proposed canal.

The second line is to the port of Realejo, which is properly an inlet formed by the junction of the Doña Paula and Realejo Rivers, and protected on the side of the sea by the islands of Cardon and Assasadores, and a bluff of the main land. It is safe and commodious; the water good, ranging from three to eight and nine fathoms. The volcano of the Viejo, lifting its cone nearly seven thousand feet above the sea to the north-eastward of the port, forms an unmistakable land-mark to the mariner when no other part of the coast is visible. This line, starting from the nearest practicable point of Lake Managua, cannot fall short of forty-five miles in length. It has been said that the Doña Paula might be made use of for a considerable distance, but such is not the fact. There is no stream upon this line, which, as has been supposed by various writers on the subject, can be made available for the uses of the canal. The "Rio Tosta" of which they speak, by which, from its described position the Rio Telica is supposed to be meant, (for no "Rio Tosta" exists) is a small stream, insufficient for any important purpose. I can discover, said Mr. Squier, no reason why this line cannot be advantageously pursued. It has the present advantage of passing through the most populous and best cultivated part of the country, and terminating at a point already well known.

[Mr. Squier here pointed out this line on a finely painted panorama of the whole plain of Leon, from the lake to the ocean, in the centre of which

is the city of Leon, once one of the finest in all Spanish America. This was examined with great interest.]

To the third line, namely, that from Lake Managua to the Gulf of Fonseca, public attention has never been generally directed. It nevertheless seems to offer greater advantages than either of the others. The gulf or bay in question is unequalled by any on the continent or perhaps on the globe. It much resembles that of San Francisco, and may be described as an immense harbor, in which all the fleets of the world may ride in safety. Its entire length within the land is not far from one hundred miles, by perhaps forty in average breadth. The three States of San Salvador, Honduras, and Nicaragua touch upon it. All the adjacent coasts are of unbounded productiveness. It embraces several islands of considerable size, surrounded by water of such depth as to enable vessels to approach close in shore. The most important of these, from the circumstance of its size and the fact that it commands and is the key to the entire Gulf, is the Island of Tigre, belonging to Honduras. This island was the head-quarters of Drake during his operations in the South Sea. It is about twenty miles in circumference, level near the shore, but rising regularly and gradually to a perfect cone in the centre. Upon this island is situated the free port of Amapala. Its possession by any great maritime nation would enable it to exercise an influence and control over the commerce of the western part of the continent, which the possession of Gibraltar by the English enables them to exercise over the commerce of Europe. From the south-eastern extremity of this gulf extends a large estuary or arm, called the Estero Real. Its course is precisely in the direction of the Lake of Managua, which it approaches to within fifteen or twenty miles, and between it and the lake, is the plain of Conejo, which is, in fact, a part of the plain of Leon. This Estero is as broad as the East River at New-York, and has for most of its extent an ample depth of water. At thirty miles above its mouth it had fifty feet. There is a narrow bar at its mouth, upon which, at low tide, there are about three fathoms. The tide rises, however, nearly ten feet, and with artificial aid the bar could, doubtless, be passed at all times. This Estero is one of the most beautiful natural channels that can be imagined, preserving, for a long distance, a very nearly uniform width of from three hundred to four hundred yards. Its banks are lined with mangroves, with a dense background of other trees. Captain Belcher, R. N., ascended the Estero for thirty miles in a vessel drawing ten feet of water. From the head of tide-water in the Estero to the lake is not more than twenty or twenty-

five miles. In case the canal is ever built, said Mr. S., I am convinced this must be its route. The advantages which would result to such a work from terminating in the Gulf of Fonseca are very great, and so obvious that it is needless to recapitulate them.

It may, therefore, in view of the preceding facts, be safely asserted that the passage from Lake Managua to the sea is eminently feasible.

The length of the proposed line of communication, for ships, from sea to sea, can only be determined by actual measurement. Mr. Squier said that, nevertheless, he would venture to submit the following calculation:

Length of the River San Juan................	90 miles.
Part of Lake Nicaragua to be traversed........	110 do.
Length of Rio Tipitapa,......................	18 do.
Length of Lake Managua,.....................	50 do.
From Lake to Realejo,.......................	45 do.
Total,...........................	313 miles.

From this must be deducted twenty-five miles, in case the line is terminated at the port of Tamarinda. In case it should terminate in the Gulf of Fonseca, it is possible it would not be many miles longer than to Realejo. The extent of actual canalization therefore would be, to Realejo, one hundred and twenty to one hundred and forty miles; to Tamarinda, ninety to one hundred and ten; to Fonseca, one hundred to one hundred and twenty.

It is useless, said Mr. Squier, in conclusion, to enter into calculations respecting the cost of the proposed work; for, until there is a detailed survey of the entire line, it must be wholly a matter of conjecture. It has been variously estimated at from twenty millions of dollars to forty millions of dollars. Assume it to cost one huudred millions of dollars, which may be as near the truth as any other calculation, still it is enough at present to know that it is feasible, and that its benefits, immediate and prospective, will be sufficient to compensate for the expenditure of double that amount, startling as it may at first appear. It is of course impossible to calculate with anything more than approximate accuracy the advantages which, on the score of economy alone, would result to the world from the construction of the proposed canal. Its general benefits to mankind, from the augmentation of commerce, the opening of new markets, the creation of new sources of demand, and

the cheapening of all articles of import, with the consequent increase of manufactures and agricultural supplies, cannot be estimated by the narrow standard of dollars and cents. The employment which would be given to the overgrown and starving populations of Europe; the new fields which it would open to enterprise; and the diffusion of light, knowledge and civilization, which follows always upon any great improvement in the physical condition of mankind, and which increases with every saving of an hour's time or a mile's distance in the communication between nations—all these are considerations which must lead the statesman, the philosopher, and the philanthropist to regard the proposed undertaking with deeper interest than any which has yet claimed the attention of mankind. The conjunction of time and circumstances is favorable to the work; and it now seems that the "Star of Empire," which stood still for a while on the crest of the Cordilleras, is destined to pass westward to its culmination, while the giant arm of conjoined capital and labor smites through the barriers which Nature has set up between two hemispheres.

In some conversation that ensued between members, after the conclusion of these observations, Mr. Squier mentioned, as a most interesting and important fact, not only as connected with the proposed work, but with the whole question of Steam Navigation in the Pacific, that a large bed of coal had recently been discovered in San Salvador, on the banks of the river Lempa, about sixty miles from the Gulf of Fonseca. It is semi-bituminous in character, resembling the Mount Savage coal. He had sent specimens to the Department of State in Washington.*

Prof. Silliman observed that the volcanic nature of the country had been advanced against the construction of the proposed work at this point, and inquired what importance was to be attached to the objection.

* Some of this coal has been analyzed and found to contain,

Volatile matter,	05.5
Carbon,	85.5
Ashes, (white)	9.
	100.

Other fragments (*lignite*) taken from another locality, exhibited,

Volatile matter,	11.
Carbon,	24.5
Ashes,	64.5
	100.

Mr. Squier replied that the general effects of volcanoes had been much overrated; that their permanent influences were comparatively local; and that, on the line in question, there was every reason to believe the volcanic force had pretty completely exhausted itself many ages ago. At any rate, it is evident that the general features of the country, between the Nicaraguan Basin and Lakes and the Pacific, had undergone no material change for centuries. He thought the objection entitled to no serious consideration.

Prof. Johnson inquired respecting the climate, and its effect upon northern constitutions?

Mr. Squier replied, that situated between latitude ten and thirteen north, the climate is essentially tropical, but it is favorably modified by a variety of causes. Upon the Atlantic declivity it is unquestionably warmer than in the interior or upon the borders of the Pacific, more humid, and more subject to rain. The country, too, is low along the coast, with numerous lagoons and inlets, and consequently more infested by annoying insects, and more subject to fevers. The climate is, however, more salubrious than would be supposed under the circumstances. This is illustrated by the fact that in the months of March and April, 1849, a party of American emigrants, one hundred and fifty in number, spent upwards of six weeks at this point, and notwithstanding the sudden transition from mid-winter to tropical heats, not to mention inadequate shelter and indifferent food, not one was seriously affected by illness. The same party, it may be mentioned, passed up the San Juan under the worst of circumstances, suffering great exposures, and remained in the interior and upon the western coast until the middle of August, with scarcely any illness amongst them, and that little generally the result of carelessness or excess. But a single member, whose health had been shattered by dissipation at home, and whose habits would soon have proved fatal in any climate, died during this period.

The valley of the San Juan once passed, the climate is unsurpassed for salubrity by any equal extent of territory under the tropics, or perhaps in the world. The year is divided, rather anomalously to the stranger, into two seasons, the wet and the dry, the first of which is called winter, and the latter summer. The wet season commences in May and lasts until November, during which time, but usually near the commencement or close, rains of some days' duration are of occasional occurrence, and showers are common, but do not often happen except late in the afternoon, (commencing about four o'clock) or in the night.

They are seldom of long duration, and often days and weeks elapse without a cloud obscuring the sky. Probably not more than *one-half* of the amount of rain falls during these six months in Nicaragua, that falls in the latitude of New York during the same period. During this season, the verdure and the crops, which during the dry season become sere and withered, appear in full luxuriance. The temperature is very equable, differing a little in different localities, but preserving great uniformity all over the country, except in the mountainous regions. The range of the thermometer is from seventy-eight to eighty-eight; in some instances sinking to seventy-seven during the night, and rising to ninety in the afternoon. During the month of June at Grenada, the average height of the thermometer was eighty-two degrees of Fah.; in Leon, during the months of July, August, and September, eighty-three. Out of the sun, this temperature would prove agreeable to most persons. There is almost constantly a cool and pleasant breeze blowing for the most part from the N. E. The nights are delicious, and sleep is seldom if ever interrupted by the heat. For weeks together, the thermometer has marked seventy-nine at ten o'clock in the evening, and seventy-eight at sunrise. During the dry season, in December and January, the temperature is less, and the nights positively cool, though not uncomfortable. The sky is cloudless, and trifling showers fall at rare intervals.

Prof. Johnson inquired further, what kind of labor could probably be employed to advantage in constructing the proposed canal?

Mr. Squier replied that it is not to be doubted that the surveys, excavations, etc., on the San Juan will not only prove the most difficult of any section of the proposed canal, but, from the nature of things, be attended with greater injury to the health of those engaged there. The forests which line that river are dense and dank, and the removal of the trees and other vegetation, and the consequent exposure of the rich earth, the accumulated vegetable deposit of ages, to the sun, would prove a prolific source of fevers and kindred diseases. The evil consequences can only be averted by employing here, as elsewhere, the natives of this latitude, inured to labor and hardened to exposure. In fact, the principal reliance throughout must be upon this kind of laborers, who, for two rials (twenty-five cents) per day, (the standard price,) would flock in all desirable numbers from all the States of Central America. For a *medio* (six and a quarter cents) per day, each man provides his own support, without further cost to his employer. The

laboring population is eminently docile, and can soon be brought to perform any kind of simple labor, such as excavating, clearing, quarrying, burning lime, etc., in a satisfactory manner.

THIRD DAY, WEDNESDAY, AUG. 21, 1850.

(*Continued.*)

SECTION OF GEOLOGY AND NATURAL HISTORY.

The Section assembled at 10 A. M., Prof. H. D. Rogers in the chair.

Several fine microscopes, of both English and American manufacture, being present, a Committee of three was appointed to examine them and report thereon;—said Committee consisting of Prof. L. Agassiz, of Harvard, Thos. Cole, Esq., of Salem, and Dr. W. J. Burnett, of Boston.

The following papers were read:

1. On the Structure of the Mouth in Crustacea. By Prof. L. Agassiz, of Harvard.

An investigation into the nature and character of the mouth of the lower animals may seem unworthy of being brought before this body. But such studies are intimately connected with the higher principles of physiology.

Let us examine the number of pieces of a lobster. We find the tail consisting of seven pieces; and the mouth, head, and thorax, of fourteen more; so that the latter are double the former.

Now, in the caterpillar the body is composed of twelve pieces, and one for the head; but the head is divisible into three; so that we have fifteen in all. But in perfect insects we find a somewhat different disposition. The body has nine pieces, the thorax three, and the head three. The body, therefore, being divided into threes, and the multiple of three—

The question now comes, is this combination fundamental, or is it mere coincidence?

In the lobster we find three different parts, with the mouth; and here I will mention that, with the Articulata, all the processes have the same morphological meaning—a claw is the same as a mandible or an antenna, etc.

Then we have six parts in the thorax, five having gills; and the sixth, which has hitherto been considered a jaw, I now think is the last of the thoracic legs.

We have, then, three for mouth, six for head, and six for thorax; it being three and its multiples.

Let us now look to the tail, composed, as we have said, apparently of seven pieces: In the compound and last joint we find two rudimentary joints, so that it must be divided into eight; so that the tail is really composed of nine pieces.

Then, summing them together, we should find three for mouth, six for head, six for the thorax, and nine for the tail.

Can this peculiar combination be the result of coincidence? It appears not. The principles of mathematics are exemplified in nature; and when we see conditions based upon such pure principles, we are induced to the belief that an Almighty and Infinite mind was their creator, and established the beautiful laws on which they rest.

Notice of the Discovery of the Upper Jaw with Teeth, of the Iguanodon, in the Wealden Strata at Hastings, in Sussex, England. By Reginald Neville Mantell, Esq., C. E.

Although no vestiges of the Iguanodon or other colossal reptiles, whose remains occur so abundantly in the Wealden strata of the south-east of England, have been discovered in America, yet the remarkable osteological characters of the gigantic herbivorous saurian of the Weald—the Iguanodon—impart an interest to its structure that cannot fail to engage the attention of the paleontologists of this country. I therefore beg to state, that, a few days before my departure from England, my father obtained a portion of the upper jaw of that reptile, with seven teeth in their natural situation. This specimen exhibits, for the first time, the mature worn molars attached to the alveolar process; and shows that the position of the teeth in the upper maxilla is the reverse of that in the lower jaw, as my father and Dr. Melville had inferred from the form and structure of the isolated teeth. Thus the startling inference that the arrangement of the dental organs in the

Iguanodon was analogous to that in the Ruminantia, is confirmed: the lower teeth are placed with the thick coat of enamel and hard dentine on the inner aspect of the jaw, and the upper teeth have their thick enamelled surface on the outer side. Thus, the statement in the Memoir on the Dental and Maxillary Organs of the Iguanodon, published in the Philosophical Transactions, (and for which the royal gold medal was awarded in 1848,) is completely established. A portion of the zygomatic process remains attached to the specimen, and its direction resembles that observable in the Rhinoceros, and differs from that of any known reptile. Figures and anatomical details of this most valuable acquisition will be laid before the Royal Society of London.

On a New Generic Type in the Class of Worms. By Charles Girard, Cambridge.

In whatever light we may view the animal kingdom, whether forming a series, one and uninterrupted, from simple to more complicated forms, or composed of groups independent from each other, it remains as a fact, that the animal now under consideration, combines in itself, the general character of two distinct groups. Its general form is framed upon the plan of Piscicola, but in addition, it has lateral appendages, (gills) reminding us of the same organs among annelids proper.

The body is flat, elongated, terminated at both extremities by a disk, on which the animal crawls about, in the fashion of the earth measurer, caterpillar, and leeches.

Indeed the body seems more alike that of a leech, thus showing a a third group to which it bears a great analogy.

This strange combination of character evidently shows a generic type, not described, as far as I know of, and for which I would propose the name of *Phyllobranchus.*

Phyllobranchus Ravenelii, is the only species known of the genus. The specimens that I have examined are nearly two inches long. The greatest width, taken on the anterior third of the length, is three-eighths of an inch, the respiratory appendages excluded. These dimensions are taken on specimens preserved in alcohol, and more or less contracted. No doubt, therefore, that during life, when the animal is in activity, the length and breadth will be found alternately

increased and diminished. Both extremities of the body are tapering; the posterior one the most. The anterior disk, sub-circular in its form, and of three-eighths of an inch in diameter, is connected to the body by a very short and contracted, cylinder shaped peduncle. Its inner surface shows numerous small tubercles, disposed in regular lines, radiating from the centre towards the periphery. The outer surface is not entirely smooth, or does not appear so, inasmuch as the radiations are to be observed through the disk, which is rather thin and transparent. The anterior region, where the mouth is situated, has nothing to distinguish it from the rest of the body, except as being deprived of appendages. The rings composing the body are narrow, and much more distinct on the inferior surface than on the upper one, where the body appears almost smooth. Thirty-one of these rings have on both sides a membranous appendage, external respiratory organs, (the so called gills among annelids,) all of them similar in shape and structure being merely a little larger on the middle of the body. They consist of a foliated expansion with several radiating folds, the outer edge being lobed or rather fringed ; under the microscope, they exhibit a net-work of small vessels, but it is difficult to tell at present whether there are regular meshes or a few anastomoses. The examination of living specimens alone will settle this question. At the base of their insertion there exists a dilatation or bubble, seen externally when filled by the circulating fluid.

Two rings deprived of appendages come next. Then again a contraction, followed by an elliptic dilatation, composed of smaller rings, ten or twelve in number, succeeded by a small caudal and terminal oblong disk, showing the same structure, although less apparent, as the anterior one.

Nothing is known of the habits of this animal, except that it was found fixed to the body of a Skate caught in Charleston, (S. C.) Harbor, and observed by Dr. St. Julian Ravenel, of that city.

Of the internal structure I am not prepared to give any account, from the want of a sufficient number of specimens, and in a fresh condition. It is to be hoped that further attention will be given to it, and anatomical investigations be made, to illustrate a structure which we may expect to be as striking as the zoological form.

At the conclusion of this paper, Professor Agassiz stated, that it is very rare to find intermediate animals existing between types. Now this specimen exhibits characters of three groups, and thus plainly shows that these characters cannot have much value as the bases of types.

ON THE CONNECTION OF THE DEPOSITS OF COMMON SALT WITH CLIMATE. By Prof. H. D. ROGERS, of Boston.

This paper aims at showing that there is an intimate connection between the present basins of salt water and the existing distribution of the earth's climates—a connection which, fully established, promises to afford us, through a study of the distribution of the ancient saliferous deposits, much insight into the climates of the earth in the past periods. A sound geological theory teaches that the original source of the salt of the great ocean, and of all the salt lakes, was in the chlorides of the volcanic minerals and rocks of the earth's crust. The action of the descending rain is to decompose these rocks, and to dissolve and float away into the receptacle of the sea the soluble salts which they contain. The geological revolutions, shifting at successive times the waters of the ocean from their bed, have laid dry a portion of the sediments, leaving behind a part of the sea water to be evaporated, and impregnating the strata with its saline ingredients. Thus, we find, that all the marine deposits, however far removed at present from any ocean, contain an appreciable quantity of sea-salt. In those climatic regions of the globe where the prevailing winds are excessively dry, and in those alone, do we find the inland Caspians, receptacles of water without outlets; and all these Caspians, without exception, are basins of saline water. The reason of this is very obvious. The constant drainage of the circumjacent districts, bringing into these insulated basins fresh accessions of saline matter dissolved or leached away from the strata over which they flow, and an arid climate, where the evaporation can carry off the surplus water, and prevent its flowing on into the general ocean, are the conditions for accumulating, in these receptacles, this constantly growing supply of salt. By this equilibrium between the drainage and the evaporation, the waters of insulated lakes become at last so strongly impregnated as to deposit or crystallize the salt upon their margins. Following up the same general fact of the incessant solution of the rocks, we behold in the great sea itself, a basin, like the other salt ones, which has no outlet for its surplus supplies but back again into the atmosphere by evaporation. Looking, then, at the primeval condition of an atmosphere, of aqueous vapor, and at the state of the ocean just after the period when the earth's general temperature had ceased to be incompatible with the liquid state of water, it was a fresh ocean and not a salt one.

Professor AGASSIZ, upon the conclusion of Professor Rogers' obser-

vations, remarked that the facts and views unfolded did, as the author said, furnish a new means of interpreting the ancient climates of the globe. From the fossil vegetable and animal organic remains, geologists have long felt themselves provided with sensitive indexes of the past temperatures of the earth at different periods, but now they have been furnished with a species of hygrometer.

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5. On Utricles as the Primordial Forms of all Animal Tissues. By Dr. W. J. Burnett, of Boston.

With the promulgation of the cell-doctrines of Schwann and Schleiden, morphological science immediately acquired a basis for it progress, such as had long been sought for.

All the organic sciences then took a wider stride in advance than may again be seen for some time to come.

Here was a fact attained, grander than any that had before or has since occurred, because it admitted of a generalization more wide, and an application more apposite, than any other.

But it has been here, as with other great and marked discoveries in organic science.

A fact of such universal application, and seemingly serving as a foundation for such an extensive series of phenomena, was allowed to rest as such, because it was such, and a vast number of facts allowed to pass unnoticed, because they were irreconcilable with the conditions of the grand principle evolved.

Since the days of these cell-doctrines, it has too often been the case that physiologists have been content to know that this or that structure in question, was based on cells of a peculiar or common kind.

From the universality of the law of cell-basis, there came the denial of the statement, that organized forms may exist without primitively existing in the state of nucleated cells.

And thus, there have been constantly what may be called *extreme* opinions, which have been allowed a certain scientific value, but which certainly knew no real scientific basis.

Of later days, when better instruments are at hand, and when observations have become more numerous, and have been more successfully conducted, results have been attained, which, as they belong to a lower stratum (the Eocene, if I may thus figuratively express myself),

may confidently be expected to induce a greater unity of opinion on these subjects.

At the last year's meeting of the Association, I had the pleasure of presenting a paper, containing the results I had attained upon the morphology of animal cells.

The prominent view there advanced was the identity as to nature of the cell and nucleus, in opposition to the opinion of Schwann, who, as is well known, has regarded them as entirely dissimilar. I maintained that the nucleus, at the earliest possible period we could perceive it, was a very minute granule—solid—but which, as it grew by aggregation, became an utricle; which last, by expansion, widened into a cell, inside of which was subsequently formed a nucleus by the condensation of the individualizing contents. Since then, I have met with no facts in my own observations in opposition to these views; on the other hand, a quite extended series of investigations, particularly in matters of pathology, has led me to be more strongly impressed as to their truth.

But by the aid of one of the best microscopes (Mr. Spencer's, of N. Y.), a still farther step has been taken, and we are now able to rest on the confines of the organized world—to watch, as it were, the first dawning rays of life, as they burst from the impenetrable darkness behind it.

The formation of cells from nuclei, or rather that they were formed from nuclei, has been a point well settled; but the nature and conditions of these nuclei or granules have been left for better instruments to determine.

The question now is, what is the nature of these granules, which we constantly meet with in connection with cells and cell-formation?

Hitherto, they have been regarded as a very minute portion of matter, agglomerated in a spherical form, which ultimately became solid, and gave rise to the cell-membrane.

By the piercing light of our best microscopes, they are found to be utricles, or minute hollow spheres, having a membrane and contents. Granular matter, therefore, used in connection with cells, has this definition, this being the point on which unorganized and organized forms meet.

By the experiments of Dr. Acherson* upon the relations of oil and albumen as the basis of all cell-forms, the marked tendency towards the formation of membranes when these two substances are brought in contact, has been shown.

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* Müller's Archiv für Anatomie, Physiol., &c. 1840. p. 44.

The universality of fat, and its presence in every tissue, make it a most convenient basis, and it is highly probable that each of these so-called granules is a minute particle of fat, which has gathered around it a membrane formed by the union with albumen, existing in a free state. When these granules are in their earliest condition, the membrane can only be inferred; but as they *grow*, its presence is unmistakable.

If, therefore, we consider them as utricles, instead of solid granules, the subsequent behavior can be well understood—such as their growth from almost indefinite points to distinct vesicles, and this by endosmosis hrough the albuminous membrane, and their arrangement under conditions reminding one of well-formed cells.

Cells cannot be considered as such in a physiological point of view, because they seek to lose their individuality in tissue-formations. This can be considered only as an expression of their vitality, so that other forms, but having the same constituents, will do the same.

So it is with these primordial utricles. They are, in a physiological point of view, cells; and thus we see that in many cases, and especially in pathological products, they arrange themselves in linear series, forming the well known *fibrillated* tissue.

In pathological conditions of organs of an inflammatory type, instead of the healthy plastic blastema being thrown out, in which arise granules which immediately ascend to the condition of active cells, there is another, porous in quality, and the granules of which, apparently wanting the power of cell elevation, remain as such, but they still evince their vital nature by forming from linear series, the fibrillated tissue above mentioned.

In the healthy economy, this same fact is brought out, in the production of many basement tissues, and that of the tails of the spermatic particles.

Although the view of Schwann—that all tissues are from nucleated cells—may hold good with those tissues of a higher and more animalized character, yet the tendency of profound microscopy is to show that these primordial utricles play an equally important part in the more ordinary tissues.

As an instance of this, I need only cite the muscular tissue forming a great part of our body. Although arising, as Lebert has shown, from large cells, yet all its condition as true muscle is when it is in a state of these granules or utricles; it is supported as such and nourished as such, and with a nicety of adjustment equalling that of the brain or any other purely cellular organ.

Now, when this *utricular* tissue is a pathological product, it may, I think, be considered as an *infra formation;* and on the other hand, when it is the result of cell-elimination, as occurs for the most part in healthy tissues, it may be considered as an *extra formation.* This view I am obliged to take, from the following facts:

In the first place, when a pathological product, and produced without a definite end or object, it seldom attains to the capacity of any function, excepting that of a mechanical basis, on which the other forms may rest. In fact, its relations seem to be scarcely above those of the minute inorganic cutaneous matter often found. Moreover, it is constantly liable to liquify, that is, to return to the simple utricular condition, and then is discharged as foreign matter. Such is the case with most inflammatory exudations, the various forms of carcinoma, and some conditions of tubercle.

On the other hand, with healthy tissues we generally meet with this *utricular* tissue in conditions where there seems to be an advance of vitality above that which would come from the cells from which these utricles are formed.

Thus in the muscular tissue, its peculiar force does not appear until its striated character is formed by the production of these primordial utricles in the parent fibre-cells. But most strikingly is it seen in the formation of the embryo and spermatic particle. The vitalization in these cases seems to increase by the reduction of cells back to utricles, from which the primary tissues arise, the utricular condition being most marked at those parts where the "animality" is highest, such as the nervous system.

In the production of the caudate portion of the spermatic particle, (as I have elsewhere shown,) the tail, evidently composed of a longitudinal series of minute utricles, exists always in a ratio corresponding with the vitality evinced by the particle. Thus it is largest and most perfect at the sexual period, and immediately breaks away with the death of the particle.

On the whole, I think it may be affirmed that our relations with the external world are brought about by *utricular* tissues; while those of the inner and vegetable world, such as assimilation and secretion, belong more properly to cell-structures.

In thus treating of those points where the organized meets, as it were, with the unorganized world, we are led to repel those charges put forth by certain physiologists, as to the complete identity of the elementary forms of the organic and inorganic kingdoms. They have

asserted that they are both composed of granules of identical character, and therefore that the inorganic may pass to the organic, by a mere condition of circumstances, which are external and accidental, in the ordinary acceptation of the word.

This, however, is wholly incorrect, and belongs to the days of imperfect instruments.* The distinction we can make, is, that the granules of the organized and organizable kingdom are *utricles*, while those of inorganic matter are solid particles.

With these bases, the nature of cell-membranes is made tolerably clear. Without doubt the albumen plays the grand part in their formation—it is the part vitalized—and it possesses the characteristics of the part producing it. The oil must be regarded as the basis on which the structure is built, the framework on which are reared those processes ending in all the material manifestations of life.

The membrane thus formed is structureless, the result of the meeting of materials possessing a mutual affinity, yet having heterogeneous peculiarities. Once formed, it possesses that peculiar property of *endosmosis* and *exosmosis*, which without doubt lie at the bottom of all material organic changes.

These membranes are capable of great dilatation, and thus being thinned, they become almost entirely transparent; which is particularly the case with those processes concerned in the appearance of life in embryology, a difficulty of no small import in the study of these principles.

When the cell-processes are more slow, and the forms more persistent, thickening of these membranes occurs, as is well known, by the deposition of granules on their interior. These granules or minute utricles, vitalized by their presence in the cell-sac, and thus deposited, are most probably the cause of those anomalous *cell-movements* not unfrequently seen. I know of no facts which would go to show that the *simple* cell-membrane possesses a contractile power. On the other hand, we have

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* It may be said that, even with the best glasses, we must arrive at a point with these particles where they will appear solid, and therefore that it is really only one step ahead. This is not, perhaps, true; for in the first place, if the limit of our power still show utricles, we have something of a right to infer that the still smaller ones are so. Moreover, there is probably a limit as to the *numerical* combination, which may not be beyond our reach, viz., points of 1-150,000 to 1-200,000 of an inch in diameter.

many instances of granules or utricles possessing that power, not only *singly*, but in combination.

The *Acherson mode* of the formation of cells (involving this subject of primordial utricles, because it is a similar process with the one he described) has been severely objected to by several physiologists. They do not doubt the fact of their formation, but doubt that, thus formed, they are capable of progressing any farther—in other words, that it is a non-vital act. There is here, I think, a two-fold error. In the first place, the formation of a membrane in these cases is a purely physical act—not vital, in the ordinary acceptation of the term.

On the other hand, it is not for a moment supposed that the conditions of its formation are everywhere the same. If oil, in the shape of very minute particles, be mixed with albumen, it cannot be supposed that such formation is identical with the production of similar forms in an active economy. In the latter case the albumen is, if we may so express ourselves, *vitalized*, and the membrane thus formed possesses the grand vital properties of endosmosis and exosmosis.

We need never fear that the organic functional world will be blended as to its elementary forms with the passive chemical conditions. And notwithstanding the characteristic tendency of physical science at the present time, it must be remembered that it still remains to be proved that organic science deals only with *material* premises, actuated by chemical laws. Our best microscopes can show no difference between the character of that albumen which forms particles that go on to the production of the highest expressions of life, and that, on the other hand, which stops on the points of its first formation.

Exactly so is it with the different characteristics of life. No difference as to material form can be detected between those primordial utricles, the ultimate end of which is the production of a *human being*, and those which produce a worm.

I know very well that such views are very liable to the name of *unphysical*, but they are so only on the ground that *physical* science knows nothing but material forms, and the chemical laws governing them, a question which certainly, at the present stage of organic science, is very far from being ripe for decision.

6. On a New Type of Scales in Fishes. By Prof. Louis Agassiz, of Harvard.

[*Not received.*]

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7. On the Relation of the Distribution of Lice to the Different Faunas. By Dr. W. J. Burnett.

In the first place, the very existence of these lower forms upon the higher animals is a zoological fact which meets with no ready accordance with the commonly received notions of the successive appearance of animals as based on geological data.

Nevertheless, if we consider the relations of dependence here sustained, we can justly infer their creation or appearance subsequent to that of the animals on which they live.

This point may be thought to involve another, viz., the doctrine of Equivocal Generation.

But this is not so, since *their* presence on animals is a zoological fact of equal importance in every relation, with the existence of the higher animals on their peculiar habitats.

And it may here be said, once for all, that, at this day of profound and accurate zoological research as to the primal conditions of organized forms, every step taken, goes for the disproof of this hypothesis. We have no right, as men of science, to leave the ground of analogy and reason, concerning points of which we are still ignorant, directly against the tenor of all our positive knowledge.

The fact, then, of the presence of these parasites upon the higher animals is a point in science of the no less value because we do not yet fully understand it in all its primal relations. It is enough for us, especially in the present case, to take it as it is, and look to the fair inferences which follow.

Although these parasites form a part of our *fauna*, speaking in general terms, yet they also may be considered as forming (if I may be allowed this mode of expression) a part of the *fauna* of the animals on which they live; for the relations that the different species sustain to the animals on which they reside are in most respects the same as those of this very animal to the terrestrial district in which *it* lives.

Now, with the general *fauna* of the earth, the fact that totally distinct genera and species exist exposed to the same vicissitudes of exter-

nal life, has been a just ground for the argument that they are not probably the result of a succession of metamorphotic changes of a single type, brought about by external influences, but they were created as such, all the types preserving their primitive integrity. This same argument is equally true in regard to these parasites.

We have not only different species, but entirely different genera upon a single animal; and as their habits are the same, and as they are exposed to precisely the same changes of external life, we have reason to infer from this fact that here were created, as in the former case, the distinct types which they now constantly perpetuate.

With these preliminary details, we come now to a consideration of the two main points of our case.

How does the existence of parasites bear upon the questions—1st. The production of genera and species from single family types. 2d. The primal existence of all the world's animals in a single locality, or their special local creation in the habitats which they now occupy.

As to the 1st question—of genera and species—

If it be true, as has been asserted by a certain class of naturalists, that the diverse genera and species of a family or families are but so many expressions of the modification of the primitive family type, and therefore appeared subsequent to the latter, we could justly expect an identity of parasites and a uniformity as to *their* special characters throughout all the species of the various genera of that family. For instance, if the species of the different genera of the family of squirrels (*Sciuridæ*) are but the modifications of a single primitive type squirrel, we should expect not only that there would be certain species in common upon all those which had parasites; but that upon closely allied species, with which, if the hypothesis were admitted, the period and process of transition would be short, there would be a uniformity as to the specific character of these parasites, and that widely separated genera of them (parasites) would not exist. But this is not true; and there is not, as far as my own experience goes, sufficient of that opposite character of the two combined classes of phenomena, to justify in the least this hypothesis of modification of the higher animal types.

According to my observation, it would appear, on the other hand, that, with the mammalia and birds on which this class of parasites reside, although there are, in many cases, certain species of parasites which are constant throughout the whole family, yet there is a well-marked tendency for each species of these higher animals to have its own particular species of parasite.

Now, from the similarity of manners of birds or mammals of the same family, their habits of close association, all these would make it possible for one species of parasite to be spread throughout them all; but on the very same premises it is scarcely conceivable that the same species of parasite should be found upon these higher animals, of very diverse families, and with which the habits are entirely dissimilar—there being no liability to any connection or association. This, however, is the fact, and as such it is not reconcilable with the hypothesis of the successive production of types by a series of metamorphotic changes of their structure. On the other hand, it would tend to show, as much as any single fact well could, that the existing specific types were created as such.

As to the 2d question—of the local creation of genera and species—it solicits still more our attention. It would be unnecessary for me here to enter upon the many points and relations involved in the question of the geographical distribution of animals.

It is only required, in behalf of what concerns the present consideration, that I should briefly allude to the most general points.

The existence of the world's animals on its surface has not that commonness which at first might be supposed. They hold relations of a local nature, which is connected with a remarkable diversity of forms.

Their very conditions in the localities they exist involve a long series of phenomena, which, at the present time, are not thoroughly understood.

At any rate, the fact is certain, that each particular region of country has a marked tendency to have *fauna* of its own.

These countries are generally marked out by climatic relations, and bounded by what are termed impassable barriers, or conditions of the earth's surface, over which animals are not likely to pass. For, were it not so, animals would mingle; and, whatever may have been their primitive origin or condition, there could, after a term of time, be no such thing as a geographical distribution of many or even most animals. Climate, undoubtedly, has a greater influence upon the character of the *fauna* than any other.

And we see in the same zone, separated by impassable barriers, a tendency towards a similarity of animal productions; although there exists, at the same time, a diversity sufficient to refute the idea of a common origin. Thus there is considerable similarity existing between the *fauna* of the United States and Europe. But each has peculiar species, and the others have been considered as deriving the name of different, although closely allied species.

This same might be said of other regions and countries, and even of the northern and eastern portion of our country, the chain of Rocky Mountains serving as the barrier.

With these peculiarities in the general distribution of animals—different countries having analogous species—the question is raised as to their common origin; or, on the other hand, their local creation in their habitats.

The facts of Geology, the history of the world's surface, and our increasing knowledge of the intimate relations of animals to the circumstances in which they live, all go to the settlement of this question on the last hypothesis.

On the other hand, it has been argued that the difference of terrestrial conditions has been sufficient to produce the differences here noticed. The relation of parasites here comes in, as affording some considerable argument against this view.

Even admitting that these differences of terrestrial and climatic conditions may produce these differences of species, it cannot for a moment be supposed that there is any, or could be any differences of existence with the parasites of these higher animals. It could not, for instance, be supposed that the lice living on the European birds would be different from those of the analogous species of birds in America. At least there are no physical conditions of existence, in the one case, not in the other. If, therefore, these higher animals arose from a common stock, their parasites should agree as to species.

This, however, is not the fact. On the other hand, not only do the parasites of our animals, compared with those of the analogous animals of the other continent, have differences, often more wide than those of the animals on which they live, (in other words, the species are more distinct than the higher animals,) but even our peculiar species of birds or mammals, having no representatives in the other continent, have their own parasites, as distinct as themselves.

Again, those animals which, by their powers of locomotion, or by human means, have become common to both countries, such as many of the domestic animals, have parasites, as far as my observation goes, identical in character. The lice of our cow, horse, or hog, for instance, do not differ from those of the same animals in Europe. The same might be said of some birds.

The general and legitimate inference from these facts is, I think, this, that the analogous species of animals of the different continents were created as such, and therefore have their proper parasites, instead of emanating from parent stocks.

And, whatever may be urged in behalf of the hypothesis of the unity of the animal creation, based upon the alleged metamorphosic changes of types, it is my opinion that the relations of their parasites, and especially the lice which are distributed over nearly all of them, must be considered as fair and full an argument as can be offered against such hypothesis, for it is taking up the very premises of the hypothesis in its opposition.

It may be asked how far these views may be pushed with regard to the higher mammalia and man.

Many of the higher mammals, man has gathered around him in domestic life; and as they go wherever he penetrates, there can be no reliable conclusions in this case.

The same may be said of man, who, fortunately, is annoyed by but few species of these parasites; man's ubiquity, and the fact that he knows no impassable barriers, make the conditions of his existence different from those of any other animal.

There are, however, some few facts respecting these parasites as they exist on man, which may be taken for what they are worth, as bearing on the question of the Unity of the Human Race, or its Diversity, according to the same line of argument adopted above in regard to the animals. The first question is,—Have all the different races of men the same, or different species of parasites?

The earliest account of these animals which we meet, is in the Old Testament, among the Egyptians; and as these were most probably a branch of the Circassian race, all our subsequent knowledge of the history of these animals with man has particular reference to the white races.

The evidence of travellers who have penetrated new countries does not bear accurately upon this point; as most of them were not entomologists, they could only state the fact, as they have done, of *lice* being present.

However, we have the authority of the eminent Latreille, that the head-louse of the native African is sufficiently different from that of the Circassian to entitle it to the rank of a distinct species.

It is much to be regretted that more and accurate observations have not been made on this point.

The lice of the *Quadrumana*, or Monkeys, seem to be quite different from those of man. In fact, the species of man are not found on the monkeys, except when their presence may be referable to accident; and those of the monkeys are *only* found there.

(As a matter of science, and as illustrative of the views here put forth,

it would be proper that I should present a tabular arrangement of these relations of parasites. But although easily done, it would be only an array of generic and specific names for your consideration, without, perhaps, making any clearer these views, which, as results, I have had the pleasure to bring out.)

THIRD DAY, WEDNESDAY, AUGUST 21, 1850.

(*Continued.*)

SECTION OF PHYSICS AND MATHEMATICS.

In this section the following papers were presented :—

1. On a New Method of Observing and Recording Astronomical Right Ascensions and North Polar Distances. By Prof. O. M. Mitchel, of Cincinnati.

Prof. Mitchel remarked, that at the last annual meeting of the Association, he had given some outlines of plans which he had contrived for observing and recording astronomical observations, by the use of the electro-magnet and other auxiliary apparatus. He now proposed to confine himself almost entirely to the examination of the results recently obtained by the new machinery, in the determination of North Polar distances. He stated that the method of recording the clock-beats and minutes of transits of stars in the transit instrument, by the electro-magnet, had been in use now more than a year. During the whole of this period the delicate fibre of a spider's web, which was so arranged as to unite the metallic lever to the pendulum of the clock, by means of which the clock is made to record its beats, had never been changed, and throughout this time not a solitary second of the clock had ever been lost through a failure in the formation of the circuit by which the electric current is transmitted.

He then explained the three prime adjustments of the revolving disc, on which the time and observations are recorded. In the course of his remarks, he had been led to notice the fact that the minute interval of time between the forming of a metallic circuit, by which an electric current is transmitted through an electro-magnet, and the response of the magnet, is not only appreciable, but variable, and depends on a

variety of conditions. This interval he denominated armature-time. To give increased accuracy to his observations of right ascension, he had found it necessary to measure this armature-time and to determine its variations. This had been readily accomplished by means described, and it had been found that the armature-time of his recording pens varied, from an almost inappreciable quantity, up to three or even four hundredths of a second of time during the same night.

After a few other explanations, the Professor proceeded to give an account of his new declination machinery. He had abandoned the use of a divided circle of zenith, or horizontal, points in the determination of north polar distance. By the new method he measured directly the diameters of the circles described by the circumpolar stars around the pole, when this diameter did not exceed 25 degrees or 30 degrees of arc. These stars being well determined, they became standards for the reference of those objects more remote from the pole.

A comparison was instituted between the American observations of N. P. D. and those made at the Royal Observatory, Greenwich, England, in 1840, in the following particulars:—

1. The relative accuracy of the observations on a single wire.
2. The number of observations which can be taken in a given time by the new and old methods.

It was found, by an extended comparison of observations on many objects, such as the diameter of the sun, the N. P. D. of the north star, and the intervals in declination of many standard stars, that in every comparison the new measures had the advantage in accuracy over the old.

While this fact existed, it was also shown that while Greenwich recorded a single observation, the new method would enable the observer to record ten times that number.

It had been found by an examination of the recorded observations, during 1840, at Greenwich, that, by the new method, the observer at Cincinnati had been able to accomplish a greater number of observations in a single month than had been made in a year at the Royal Observatory. The increased accuracy was accounted for, by the more perfect means afforded in the new methods of reading the pointing of the telescope, while the greater number of observations arose from the introduction of the principle of repetition.

The Professor closed by adverting to the fact, that the new machinery was exceedingly imperfect, having been, to a great extent, made in

the Observatory by himself and his assistant, who were not mechanics.

Specimens of the engravings executed by the new apparatus were exhibited to the Association.

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2. On the Laws of Perfect Musical Intonation. By H. W. Poole.

[Not received.]

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3. Elliptical Tables of the Planet Neptune. By Prof. George W. Coakley, St. James' College, Maryland.

The Elliptical Tables of the Planet Neptune, an account of which was laid before the Physical Section of the Association, are comprised in a set of nine tables, and accompanied by a full introduction, which explains their construction. The tables are based upon the second Elliptical Elements of Mr. Sears C. Walker, provision being made for correcting the tables, whenever the elements assigned by Mr. Walker shall be corrected.

The first table contains, for the beginning of each year, from 1840 to 1870 inclusive, the mean longitude of Neptune, estimated from the mean equinox of Jan. 1st, 1850. These mean longitudes may readily be extended backwards or forwards as many years as we please, by the simple addition or subtraction of certain constant numbers. The second and third columns of the same table contain, respectively, the longitude of the Perihelion and that of the Node. They are constant for the constant ellipse, and can only vary by reason of the perturbations, which, when ascertained, are readily applied to these constants.

The second table contains the corrections for precession, which must be applied to the mean longitudes of the first table, in order that those longitudes may be referred to the mean equinox of the year for which the place of Neptune is wanted. Tables III., IV., and V., give the mean motions for months, days, hours, minutes, and seconds, by the application of which to the mean longitude for the commencement of the year, the *mean place* of the planet becomes known for any proposed time. If to this *mean place* we add the correction, called the *Equation of the Centre*, we have the place of the planet in its elliptical orbit. This correction is furnished by Table VI., with great

precision, for any time. The place thus found is the longitude on the planet's orbit; but this being corrected by the reduction in longitude from Table IX, we have finally the true longitude of the planet on the ecliptic, with the exception of the perturbation in longitude.

Table VII. gives the logarithm of the planet's radius-vector, and a reduction from Table IX. being applied to this, when desired, we have the logarithm of the projection of the radius-vector on the ecliptic.

Finally, Table VIII. gives the heliocentric latitude for any time. Thus we know for any moment the heliocentric co-ordinates of the planet Neptune considered as moving in a constant ellipse. If, however, by any means whatever, we get at a knowledge of Neptune's perturbations, we have only to apply these to the results before obtained in the way just described, and we then have the absolutely true heliocentric co-ordinate.

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4. Ice of Lake Champlain: why it all disappears at once. By Prof. D. Olmsted, of Yale.

[*Not received.*]

Prof. H. D. Rogers remarked that the sudden disappearance of the ice was readily explained, by its previous state of aggregation. The ice, during an alternation of thawing and freezing, resolves itself into a series of slender vertical columns loosely coherent, and themselves readily separating into small grains. In this state of instable equilibrium, among its parts the floating sheets of ice are liable, at the slightest impulse, to break up into granules or small pieces, which, settling down suddenly in the water, although they do not really sink to the bottom, cause the ice, as a mass, to disappear. This instability of aggregation has been observed in icebergs which have floated south; so that the concussion of the air, produced by firing a gun, was sufficient to cause the mass to break up at once into small fragments, and thus suddenly to subside.

Dr. Hare was of opinion that, however ice might be perforated or comminuted, the residue would be found of the same specific gravity as the mass out of which it had been formed, and, consequently, to whatever extent it might be penetrated by water, the resulting mass would be as much lighter than its bulk of water, as the ice comprised

by it should be lighter than water; moreover, as below the temperature of 39 deg., the density of water diminishes as it becomes colder, at the temperature which this liquid must have within the interstices of an icy mass, it would not, of itself, sink through the warmer subjacent aqueous strata, and, of course, it could not cause any ice with which it may be associated to sink. This buoyancy of water near the temperature of congelation had been considered as a most interesting and important provision of nature, by which the consolidation of large masses of water in the polar regions was prevented, when otherwise it might take place so as to be beyond the influence of the summer sun to restore it to liquidity. But, as in a mass of water cooled throughout to 39 deg., the colder portion must be lighter, and consequently supernatant, this portion together with the ice into which its superior portion is converted, forms a sort of winter clothing, which checks the growth of the ice with an efficacy which augments as the ice thickens.

Dr. HARE concurred with Prof. H. D. ROGERS in the idea that the icy mass, covering a body of water, might become so extremely friable as to be dissipated by the wind into very minute fragments, and suggested, in addition, that when thus disseminated, the particles would be speedily liquified by contact with the warmer water below.

It has been suggested that muddiness of water would render ice from it more capable of receiving heat from the sun, and more friable.

THIRD DAY, WEDNESDAY, AUGUST 21, 1850.

(*Continued.*)

SECTION OF CHEMISTRY AND MINERALOGY.

The Section met at 10 A. M. in the Trumbull Gallery. Dr. TORREY, of Princeton, was called to the chair, and Dr. R. E. ROGERS, of Virginia, appointed Secretary to the Section. The following communications were presented:

1. An Account of some Experiments upon the Cause of Fermentation. By Prof. H. Erni, of Yale College Laboratory.

The experiments and observations on Fermentation here communicated by Mr. Erni were made in the course of the year 1847, in the Chemical Laboratory of Zurich. Their publication has been deferred because Mr. E. left Switzerland before they were extended and perfected so much as he had desired. It is well known among chemists, that, notwithstanding the voluminous writings on Fermentation, we yet know very little of its original cause, and no one of the theories advanced is competent to the full explanation of the facts known at present. It is doubtless true that in this process, by the action of the so-called ferment, organic bodies of complex composition, such as sugar, &c., are decomposed into simpler substances.

The cause of these phenomena is considered by Liebig to lie in the power of the ferment as a body in a state of decomposition, *i. e*, in chemical action, to induce a similar transformation of compounds that come in contact with it. Other chemists assume that fermentation is caused by the development of fungi, and that different kinds of fermentation are due to different fungi.

The results which Mr. Erni has obtained, a part of which have been fully corroborated by more recent examinations, have enabled him to throw considerable light on some disputed points. A great variety of experiments were published by Brendecke, according to which, porous substances, as straw, feathers, alum, pulverized charcoal, potato starch, flowers of sulphur, scraps of paper, and even small fragments of some metals, as tin, for instance, induce fermentation in a solution of grape-sugar to which some tartrate of ammonia has been added. After this it seemed probable that yeast, consisting undoubtedly of vegetable cells, might, like the substances mentioned, produce fermentation in solutions of cane or grape sugar, not by means of the vital force, but simply by its looseness and porosity. In repeating some of Brendecke's experiments, Mr. Erni had used grape sugar, prepared from honey by means of alcohol, and purified by boiling with charcoal, and cane sugar prepared from white refined sugar re-crystallized from solution in hot water. In the experiments one part of sugar and eight of water were employed.

1*st Experiment.*—In the first experiment, common straw was treated with potash ley, to dissolve any gluten it might contain. After twenty-four hours the straw was washed with water, the last traces of potash

removed by hydrochloric acid, and finally the straw perfectly cleaned with water. The solution of cane and grape sugar mixed with such straw underwent no change during four weeks.

2d Experiment.—Additions of tartrate of ammonia to such solutions as above, produced no perceptible effect.

3d Experiment.—Straw, cleaned as mentioned, together with cream of tartar, induced no fermentation in either cane or grape sugar.

4th Experiment.—Pulverized quartz, added to solutions of cane and grape sugar, caused no fermentation. In the course of some weeks, mucor or mould was formed, which fructified and yielded spores. Such was the case not only in this, but also in some of the previous experiments.

5th Experiment.—A solution of cane sugar was mixed with uncleaned natural straw, in order to see if it excited fermentation, and if so, to ascertain if it may not be owing to the development of fungi. After twelve days, fermentation commenced, and at the same time microscopical examination of the liquor showed the presence of vegetable cells identical with upper yeast.

6th Experiment.—The same experiment performed with grape sugar yielded the same results, but fermentation commenced some days earlier.

Experiment 7 to 11.—These experiments were made upon yeast. Rousseau made known the supposed fact, that fermentation may be induced by yeast, even in the presence of vegetable or mineral poisons, if rendered acid when mixed with the sugar solution. The trials of Mr. Erni did not confirm these statements. He found—

1st. That upper yeast, in cane sugar solution, acidified with tartaric acid, and poisoned with arsenious acid, produced no fermentation. The same mixture, without the addition of oxide of arsenic, fermented after two days. The liquor was strongly acid in taste, became sweet again, and fermentation commenced.

2d. The same trial was made in a liquor acidulated with acetic acid, and no fermentation took place. The same mixture without the oxide of arsenic produced fermentation.

3d. The above two experiments were repeated, using oil of turpentine as a poison, instead of arsenious acid, and the same negative results obtained.

4th. Yeast added to cane or sugar solution, acidified with tartaric or acetic acid, and poisoned with a few drops of kreasote, excited no fermentation whatever.

5th. Mixtures of cane and grape sugar with yeast, acidified with cream of tartar, and poisoned with considerable quantities of arsenious acid, yielded fermentation. Perhaps in this case the arsenious acid formed a chemical compound with the cream of tartar, as such a salt was proved to exist by Mitscherlich.

6th. The same experiment repeated, with the difference that in the place of arsenious acid, kreasote and oil of turpentine were employed, showed no fermentation.

When the poisoned liquors were examined under the microscope, it could be easily seen, when the poison had taken effect, the nitrogenous layer on the cell-membrane seeming to have undergone a change similar to that produced by boiling.

Mr. Erni's investigations thus far led him to the conclusion *that alcoholic fermentation is caused by the development of fungi.* He could never trace fermentation without observing, at the very first evolution of carbonic acid, the formation of yeast-cells, although it is very difficult to decide certainly which precedes the other. He was rather in favor of the yeast-cells being the original movers—the results of Mitscherlich's experiments. Helmholz has also observed that the fermentation of grape-juice is not communicated to another portion of grape-juice which is contained in a vessel closed by a bladder and introduced into the fermenting liquid. These facts, together with the experiments as to the action of poison on yeast, seem to admit of no other explanation, even by the ingenious theory of Liebig.

Mr. Erni adduced a number of his experiments on other kinds of fermentation—showing that different kinds of fermentation are due to different kinds of fungi.

At the close of the reading of this paper, Dr. Hare, of Philadelphia, remarked he had noticed that fermentation always took place between those limits of temperature, only, within which animal life can exist.

He considered Liebig's idea, that the cause of fermentation was communicated by one body to another, was only substituting mystery for mystery. He expressed his belief that galvanic action had something to do with the process.

Dr. R. E. Rogers, of Va., remarked that while he regarded it an interesting fact stated by Mr. Erni, of the power of arsenic to "poison" the yeast fungus, and thus prevent its further development and propagation, and therefore to arrest vinous fermentation, yet he did not think we could from that conclude it to be a poison to all the lower forms of vegetation; or that because a substance is a poison to animals,

(as in the case of arsenic,) that it is therefore so to vegetables. He cited the fact, that not only is vegetation freely developed in many saline solutions, but especially often in those of the arsenite of potassa and corrosive sublimate—substances the most poisonous to animal forms, and in ordinary cases remarkable for their antiseptic properties. He thought that in studying such lower forms of organization, it was necessary, as in the case of the higher, to investigate the peculiarities of each species.

Dr. Torrey thought that the enumeration of the substances in which organisms occur would be of interest. He believed that while many acids and saline solutions present them, they had never been detected in compounds of chrome.

Mr. Hunt stated that he had observed a large number of what appeared to be fungi in water taken from a spring in Western Canada, which contained two parts of sulphuric acid in a thousand, besides hydrosulphuric acid gas.

Prof. Horsford remarked, that he had made a similar observation in the case of the water from the Oak Orchard Springs, in Western New York; that he had submitted the light-colored deposit obtained from bottles containing this water, to Dr. Henry J. Bowditch, of the Cambridge Laboratory, for microscopical examination, and that this gentleman had exhibited drawings of the objects to Prof. Gray, who pronounced them to be imperfectly developed vegetable organisms.

Prof. Silliman, Jr., mentioned that during last year he was consulted by a tanner of New Haven, who was engaged in the manufacture of morocco leather, in which he used the sumac, and who experienced great difficulty in his process, in consequence, as he (Prof. Silliman) believed, of the rapid conversion of the Tannic into Gallic Acid, caused by the fermentative action of animal or vegetable organisms; that he was led to suggest to him the use of kreasote in his liquors, and that it was found to arrest entirely the fermentation.

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2. On the Troostite of New Jersey. By Henry Wurtz, of New York.

In the ninth volume of the second series of the *Am. Journal of Science*, page 408, appeared an article by Mr. Dana, which leaves nothing more to be said with regard to the history of the mineral Troostite. Its

dientity with Vanuxem and Keating's silicious oxyd of zinc, and with Willemite, is now fully recognized.

My results confirm those of the original discoverers, Vanuxem and Keating, and those of Hermann, in opposition to those of Thomson, who made it out to be a tribasic silicate of manganese without zinc.

The portion which I analyzed was taken from the centre of a large crystal having the ordinary hexagonal form of Willemite, from Stirling, N. J. Its color in the mass was dark brown, and in powder light brown. This color was found to be probably owing to the presence of a trace of intermixed black oxide of manganese, indicated by the evolution of a small quantity of chlorine when the powder was acted upon by hydrochloric acid.

A determination of the specific gravity made upon the same portion which was afterwards analyzed, in coarse powder, gave the number 3·986.

The powder gelatinized very greatly with hydrochloric acid, and it was found difficult to decompose it completely in this way. It was therefore fused with carbonate of soda.

The analysis gave—

		Oxygen.	
Silica,	27·91	14·50	14·50
Oxide of Zinc,	59·93	11·83	
Protoxide of Manganese,	3·73	84	
Protoxide of Iron,	5·35	1·19	14·96
Magnesia,	1·66	64	
Lime,	1·60	46	
	100·18		

Giving exactly the formula $\dot{Z}n^3 \dddot{S}i$, a portion of the zinc being replaced by manganese, iron, &c.

The small excess of the oxygen of the protoxides over that required by the formula, is sufficiently accounted for by the presence of the intermixed deutoxide of manganese.

3. On American Spodumene. By George J. Brush, Analytical Laboratory, Yale.

Owing to the want of a complete analysis of an American Spodumene, I was induced, at the suggestion of Prof. Silliman, Jr., to undertake this research.

The Spodumene from Utö has often been the subject of chemical investigation, and has been analyzed by Arfvedson,* Stromeyer,† Regnault,‡ and Hagen.§ That from the Killiney locality has been analyzed by Thomson.‖

These are all the complete analyses recorded of this species. Partial analyses, however, exist of specimens from the Tyrol mountains, and from Sterling, Mass., the former by Hagen, and the latter by both Hagen and Bowen.¶

The constitution of this mineral was not correctly understood prior to Hagen's analysis, until which time it had been considered as essentially a silicate of alumina and lithia. Hagen, however, found a portion of the so-called lithia to be soda, which discovery being confirmed, renders the formulas derived from former analyses incorrect, owing to the great difference in the atomic weights of lithia and soda. Hagen's analysis of a specimen from the Utö locality gave,

			Oxygen.		Ratio.	
Silica,	66·136	=	34·36	34·36	12·26	12
Alumina,	27·024		12·63	12·72	4·55	$4\frac{1}{2}$
Peroxyd of iron,	·321		·09			
Lithia,	3·836		2·11	2·79	1	1
Soda,	2·683		·68			
	100·000					

from which he deduced the formula $\dot{Na}\,\dddot{Si}+3\dot{Li}\,\dddot{Si}+6\dddot{\mathbf{Al}}\,\dddot{Si}^2$.

My analyses agree with Hagen's in the soda, but lead to a different formula. The specimens selected for analysis were from the Norwich and Sterling (Mass.) localities. A qualitative examination of each showed the presence of silica, alumina, peroxyd of iron (trace), lime, lithia, and soda.

In the quantitative examination the alkalies were obtained by decomposition by hydrofluoric acid and determined as sulphates; the other

* Schweigger's Jour. xxii. 107.

† Untersuchungen, i. 426.

‡ Ann. des Mines, (III series) 1839, 580.

§ Pogg. Ann., xlviii. 371.

‖ Thom. Min., i. 302.

¶ Sill. Am. Jour., viii. 121.

constituents were obtained by fusion with carbonate of soda. That from Norwich in two analyses yielded,

	I.	II.	Mean.	Oxygen.		Ratio
Silica,	63·06	62·72	62·39	32·67	32·67	8·04
Alumina,	28·00	28·85	28·42	13·28	13·28	3·27
Lime,	·95	1·13	1·04	·29		
Lithia,	5·67	5·67	5·67	3·12	4·06	1
Soda,	2·51	2·51	2·51	·65		
	100·19	100·88				

And that from the Sterling locality, of which also two analyses were made, gave,

	I.	II.	Mean	Oxygen.		Ratio.
Silica,	62·86	62.67	62·.76	32·61	32·61	7·80
Alumina,	28·83	29·83	29·33	13·75	13·75	3·28
Lime,	·56	·71	·63	·18		
Lithia,	6·48	6·48	6·48	3·56	4·19	1
Soda,	1·76	1·76	1·76	·45		
	100·49	101·45				

The mean of the ratios calculated from the four analyses is 1 : 3·27 : 7·92, or quite nearly 1 : 3 : 8, which gives the general formula

$$\dot{R}3\,\dddot{Si}^2+3\dddot{Al}\,\dddot{Si}^2$$

and the special formula

$$(\cdot 0570\,\dot{Ca}+\cdot 1333\,\dot{Na}+\cdot 8097\,\dot{Li})^3\,\dddot{Si}^2+{}^3\dddot{Al}\,\dddot{Si}^2$$

which requires,

8 atoms	of silica,	4618·48	=	pr. ct.	64·14
3 "	alumina,	1925·40			26.76
2·4291 "	lithia,	441·27			6·12
·3999 "	soda,	154·84			2·15
·171 "	lime,	60·10			·83
		7200·09			100·00

This formula corresponds quite well with the analyses, especially in the protoxyd bases, the mean of which is almost precisely that required by the formula.

With the specific gravity 3·18 we obtain from the above the atomic volume 2264. The B atomic volume (see Mr. Dana's memoir in *Am. Journal,* ix. 220) will be 161·7, and the C atomic volume 42·7. The

isomorphism of this species with pyroxene is pointed out by Mr. Dana on page 120 of the *American Journal of Science*, vol. x.*

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4. On the Adulteration of Vermilion. By Henry Brown, of the Cambridge Laboratory.

The following analyses were made from samples of vermilion procured in the Boston market, and probably indicate the average purity of the article in our shops.

Qualitative Analysis.

As pure sulphide of mercury volatilizes readily upon the application of heat, the fact of a residue after igniting a sample of vermilion upon platinum, is proof of the presence of impurities.

This test showed six out of ten varieties of vermilion, submitted to analysis, to be adulterated. The qualitative examination of the residues indicated the presence of carbonate of magnesia, sulphate of lime, chromate of lead, and red oxide of lead.

Ammonia salts, or other salts of mercury which are also volatile, are too expensive for the purposes of adulteration, if they admit of being so incorporated with a superior quantity of genuine vermilion as to escape recognition. None of them were found in the qualitative examination.

Quantitative Analysis.

No. 1. Trieste Vermilion. This sample entirely volatilized upon platinum.

			HgS = 100	100
No. 2.	—	—	Entirely volatilized	
			HgS = 100	100
No. 3.	—	—	HgS = 85·06	
			$MgO\,CO_2$ = 14·94	100
No. 4.	—	—	HgS = 73·43	
			$MgO\,CO_2$ = 26·57	100

* The analysis by Mr. Brush corresponds in the proportion of silica with the results of Stromeyer's investigation, who found 63·288 of silica and 28·776 of alumina. In the ratio above deduced, as well that of von Kobell's formula ($\dot{R}^3\,\dddot{Si}^2 + 4\dddot{Al}\,\dddot{Si}^2$), the oxygen of the bases is to that of the silica as 1 : 2, the same ratio as in pyroxene.—J. D. D.

No. 5.	Trieste Vermilion,	HgS = 100	100
No. 6.	French Vermilion,	HgS = 100	100
No. 7.	— —	HgS = 64·13	
		$CaO.\ SO_3$ = 35·87	100
No. 8.	Chinese Vermilion,	HgS = 51·84	
		$PbO.\ Cr.O_3$= 48·16	100
No. 9.	Chinese Vermilion,	HgS = 37·64	
		$PbO.\ Cr.O_3$= 62·36	100
No. 10.	American Vermilion, so called.		
	Pure red lead, Pb_3O_4	100	100

In color, Nos. 9 and 10 were a deep red, bordering on purple. No. 8 was lighter. Nos. 1 and 6 were lighter than Nos. 2, 3, and 5; and Nos. 2, 3, 4, 5, and 7, could not be distinguished from each other, so perfect was the incorporation of the white with the red.

The following table exhibits the relative purity of the samples analyzed, and the proportions of the articles employed for adulteration.

Samples.	Sulphate of Lime.	Carbonate of Magnesia.	Chromate of Lead.	Red oxide of Lead.	Sulphide of Mercury.	Total.
No. 1, Trieste					100·00	100
No. 2,					100·00	100
No. 3,		14·94			85·06	100
No. 4,		26·57			73·43	100
No. 5,					100·00	100
No. 6, French					100·00	100
No. 7,	35·87				64·13	100
No. 8, Chinese			48·16		51·84	100
No. 9, Chinese			62·36		37·64	100
No. 10, American, so called				100		100

5. On the Dimorphism of Copper. By William P. Blake, of New Haven.

In the course of some electrotype experiments made in December, of last year, I obtained at one time a deposit of copper, *beautifully crystallized*, in small *hexagonal* needles. The solution of sulphate of copper in which they were found was saturated, and had been made quite acid by the addition of sulphuric acid; its temperature was about zero centigrade.

The measurement of the angles of the crystals was attempted; as, however, they were quite small, and the lateral planes not brilliant, it was thought best to await the production of larger ones before pronouncing upon the system of crystallization.

I have not yet been able to make any further experiments for that purpose.

I have since seen in the collection of C. M. Wheatley, Esq., of New York, a specimen of native copper, from Corocoro, Bolivia, South America, which has the hexagonal form. The best formed crystal is nearly three-quarters of an inch in diameter, and would weigh one and a half ounces. The lateral planes are not perfectly flat, but are somewhat depressed and concave, giving the crystal the appearance of having shrunk after taking its angles.

I have not had an opportunity to give this crystal a careful examination, or to measure the angles, in order to be certain that it is not a pseudomorph. I have, however, considered the fact of its occurring in this form as additional evidence of the dimorphism of copper.

Dr. Jackson remarked that Mr. Alger and himself had examined the hexahedral crystals of copper in Mr. Wheatley's collection, and that they had come to the conclusion that the crystals were pseudomorphs of arragonite. He observed crystals of arragonite in the same collection, having almost the same dimensions as the supposed pseudomorphic crystals of copper.

He further stated, that cubic crystals of red oxide of copper are, in Cuba, not unfrequently reduced to pure metallic copper without change of form, and that he had, by means of dry hydrogen and a red heat, changed cubic crystals of red oxide of copper into metallic copper. Care must, therefore, be used to distinguish between the crystals of reduced ores, and those which are formed by crystallization of the metal. He regarded the observations which had been made on dimorphism as important, and as deserving a thorough investigation, for there were important practical questions which might receive light from it, since there were known differences of crystalline effect in copper, produced in the refining furnace by the operation of "poling" the heated metal for different lengths of time. He hoped that Mr. Blake would continue the research.

Further observations were made upon the subject by Prof. Silliman, Jr., and Mr. Hunt.

6. ON SOME SALINE SPRINGS CONTAINING BARYTA AND STRONTIA. By T. S. HUNT, of the Geological Commission of Canada.

The author stated, that these two bases had already been noticed in some of the mineral waters of Germany, by Struve. In his (the author's) examinations he had detected their presence in several of the saline springs which rise through the lower silurian rocks of Canada East, both in the form of chlorides, with chlorides of calcium and magnesium, and as carbonates with carbonate of soda in alkaline waters. To detect their presence and determine their quantity, the water was treated with a little hydrochloric acid; being evaporated to a small bulk, an equal volume of a solution of gypsum was added, and after some hours the precipitate of sulphates was collected, fused with an alkaline carbonate, and the insoluble earthy carbonates being converted into chlorides, the addition of fluosilicic acid determined a crystalline precipitate of the fluosilicide of barium; the liquid separated from this was then treated by a solution of gypsum, by which the strontia was separated as a sulphate.

The proportion of baryta was generally larger than that of strontia; in the two springs of Varrennes, where they exist as carbonates, the proportion of carbonate of these bases, in 1,000,000 parts of water, were 22 of carbonate of baryta and 14 of carbonate of strontia in the one, and 12 of carbonate of baryta and 9 of carbonate of strontia in the other.

Mr. HUNT stated further, that these waters were otherwise so interesting, that he would lay before the Section the general results of the analysis, besides those of several other springs, saline in their character, in some of which these rare bases are wanting, being not unfrequently excluded by the presence of soluble sulphates. These springs are all strongly saline, contain alkaline chlorides invariably associated with bromides and iodides, together with earthy chlorides and carbonates, not unfrequently associated with carbonate of iron, and small portions of silica and alumina. The waters containing baryta may well be supposed to have some medicinal powers, attributable to the presence of that base, and the attention of the medical profession is also invited to the probable difference between the therapeutic characters of the springs containing earthy chlorides, and those in which the existence of them is precluded by that of a true alkaline carbonate; these two classes are generally employed indiscriminately in medicine, notwith-

standing the well known active character of the chlorides of magnesium and calcium.

The communication led to a brief discussion between Dr. R. E. Rogers and Mr. Hunt, as to the probable condition of combination of the bases in this water, which contains some free carbonic acid.

In this connection Prof. Silliman, Jr., stated a case of some interest, showing the influence of carbonic acid as a metamorphic agent in changing the constitution of silicates. He referred to the analysis of an altered scapolite from New Jersey, in which a portion of the lime from the silicate has been converted into carbonate of lime, although no change has taken place in the external character as observed, except a less degree of hardness. To this, as a general cause, we must look for an explanation of numerous cases of silicates having variable quantities of carbonates of lime, many of which have been proposed as separate species.

The Section adjourned, to meet to-morrow at 10 o'clock.

FOURTH DAY, THURSDAY, AUGUST 22, 1850.

GENERAL MEETING.

(*Morning Session.*)

The Association met in general session, in the College Chapel, at 9 A. M.

Mr. E. F. Teschemacher, of Boston, presented, through Prof. Horsford, a communication from H. Taylor, Esq., of London, accompanied by 20 copies of his tract on the *Decimal System, as connected with Coinage, Weights, and Measures,* for distribution among the Members of the Committee on the subject of a uniform standard of Weights and Measures.

In accordance with the recommendation of the Standing Committee, the Report of Lieut. Davis, on the Nautical Almanac, was accepted and ordered to be printed.

REPORT OF THE COMMITTEE ON THE PRIME MERIDIAN—APPOINTED AT THE MEETING OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE, HELD AT CAMBRIDGE, AUGUST 14, 1849.

The Committee on the Prime Meridian have the honor to report, that in consequence of the remoteness from each other of the persons composing the whole Committee, all hope was precluded of a general meeting to discuss the question under consideration, and to agree upon a common report. A meeting, therefore, of as many of the Committee as could be called together at Cambridge, was held, and it was determined that a correspondence should be opened with all the members, as the best means of obtaining their views and opinions. Accordingly, a Sub-Committee was appointed, consisting of Lieutenant DAVIS, Professor BACHE, and Lieutenant MAURY, to which the duty of conducting this correspondence was assigned; and a letter signed by the Chairman of the Sub-Committee, accompanied by copies of the papers on the Prime Meridian read to the Association by Lieutenant DAVIS and Professor HOLTON, was sent to every member. The papers served to suggest the principal topics to which the attention of the members of the Committee was called, and the letter of the Chairman invited a free communication of their views and reflections. A copy of this letter is appended to this Report. Twelve of the Committee replied to the circular, of whom five were in favor of retaining the old standard, five were in favor of an American meridian solely, and two were in favor of retaining the English meridian for nautical purposes, and of establishing an origin on this continent for geographical and astronomical purposes.

These letters were transmitted to the Honorable WM. BALLARD PRESTON, at that time the Secretary of the Navy, who placed them in the hands of the Naval Committee of the House of Representatives of Congress, to which the whole subject had already been referred.

This Committee, after having the subject for some time under consideration, introduced a resolution, accompanied by a brief but able report from its Chairman, the Hon. F. P. STANTON of Tennessee, recommending that the Greenwich zero of longitudes should be preserved for the convenience of navigators, and that the meridian of the National Observatory should be adopted by the authority of Congress as its first meridian on this continent, for defining accurately and permanently, territorial limits, and for advancing the science of astronomy in America.

In conclusion, your Committee have to say, that so far as it has

heard, the mode of settling this question recommended by the Naval Committee of the House, by which the opinions entertained on both sides have been met, and partially complied with, seems to have received general approbation.

All of which is respectfully submitted.

(*Copy.*)

CAMBRIDGE, Mass., Sept. 17, 1849.

DEAR SIR,—

At the late meeting of the American Association for the Advancement of Science, held at this place, a Committee was appointed upon the subject of an American Prime Meridian, of which you were elected a member.

This Committee had its origin in the following circumstances:—

Having been charged by the Hon. Wm. Ballard Preston, Secretary of the Navy, with the duty of preparing for publication, the American Nautical Almanac, provided for by the Act of Congress, approved March 3, 1849, I addressed to him a letter concerning the Prime Meridian to be adopted in the calculation of this work, the selection and determination of which form the first step in my progress.

Mr. Preston, in his reply (dated August 7) to this communication, directed me "to bring the subject of an 'American Prime Meridian' before the American Association for the Advancement of Science, to convene at Cambridge, Mass., on the 14th instant, for the purpose of soliciting the opinions of the principal Mathematicians and Astronomers upon that highly interesting subject."

In compliance with these instructions, I submitted to the Association a paper, the same in substance as my letter to the Hon. Secretary, which, upon motion of Professor A. D. Bache, Superintendent of the U. S. Coast Survey, was referred to a Committee, consisting of twenty-two members, (whose names are subjoined), with instructions "to send a copy of their report to the Hon. William Ballard Preston, Secretary of the Navy."

A meeting of as many of this Committee as were then present was held, and a Sub-Committee, consisting of Lieut. Davis, Prof. Bache, and Lieut. Maury, was appointed to conduct the correspondence, and to execute the instructions of the Association.

The persons composing the whole Committee are so remote from each other as to preclude the possibility of a general meeting to discuss this question, and agree upon any common Report; it has been

determined, therefore, as the best means of obtaining their views, to address each member of the Committee separately.

Accordingly, I have the honor, as Chairman of the Sub-Committee, to transmit to you a copy of the paper presented to the Association by myself, and also of another paper on the Prime Meridian, by Professor Holton, referred to the Committee, and to ask your early attention to this communication.

These papers will serve to suggest the principal topics to which your attention is invited, and will, I hope, lead to a free communication of your views and reflections. It is not impossible that the letters of the Committee may be hereafter officially called for; you are requested to state, therefore, whether you object to having your letter printed, and, if no objection be given, I shall feel authorized to make it public, if required.

Very respectfully, your obedient servant,
CHARLES HENRY DAVIS,
Lieut. U. S. Navy, Sup't Nautical Almanac.

LIST OF THE COMMITTEE ON THE PRIME MERIDIAN.

Prof. A. D. Bache, Sup't U. S. Coast Survey.
Lieut. M. F. Maury, Sup't National Observatory.
Prof. Fred. A. P. Barnard, University of State of Alabama.
Prof. Lewis R. Gibbes, Charleston, South Carolina.
Prof. Edward H. Courtenay, University of Virginia.
Prof. Stephen Alexander, Princeton College.
Prof. John F. Frazer, University of Pennsylvania.
Prof. H. J. Anderson, New York.
Prof. O. M. Mitchel, Cincinnati.
Prof. A. D. Stanley, Yale College.
Hon. Wm. Mitchell, Nantucket.
Prof. Joseph Lovering, Cambridge.
Prof. William Smyth, Bowdoin College.
Prof. Joseph Winlock, Shelby College, Kentucky.
Prof. Geo. W. Coakley, St. James's College, Md.
Prof. Curley, Georgetown College.
Prof. J. Smith Fowler, Franklin College, Tennessee.
Prof. James Phillips, University of North Carolina.
Prof. Wm. H. C. Bartlett, West Point.
Prof. Ebenezer S. Snell, Amherst College.
Prof. Alexis Caswell, Brown University.
Lieut. C. H. Davis, Sup't Nautical Almanac.

In accordance with the recommendation of the Standing Committee, it was determined to hold Section Meetings during the afternoon as well as morning of Friday, and to hold a General Meeting on the evening of that day.

The call for Reports of Committees was then resumed by the President.

1.—*Committee for Memorializing Congress in relation to Scientific Explorations.*

The Chairman being absent, no report was presented.

2.—*Committee on a Change in the Constitution providing for Honorary Members.*

The Chairman was absent, and no report was presented.

3.—*Committee on Physical Constants.*

The PRESIDENT stated that Professor FRAZER, of Philadelphia, had requested him to say that illness had prevented the execution of the commission on his part.

The Committee was then ordered to be continued to the next year.

FOURTH DAY, AUGUST 22, 1850

GENERAL MEETING,

(*Afternoon Session.*)

On recommendation of the Standing Committee, it was

Resolved, That the thanks of the Association be presented to Mrs. AMOS BINNEY, of Boston, for her generous gift of numerous copies of the Transactions of the Association of American Geologists and Naturalists, for 1840, 1841, and 1842.

A resolution of the Standing Committee, that the next annual meeting of the Association be held in the city of Albany, N. Y., and that an intermediate meeting in the spring be held at Cincinnati, Ohio, was presented, but no action had thereon.

The PRESIDENT laid before the Association a printed copy of the proceedings of the meeting at Charleston, S. C., in March last, and, in compliance with the request of the members then present, he now gave, in writing, an account of the character of that meeting, and offered certain recommendations as to improvements in the organization of the Association, with suggestions concerning the relations of science and religion.

REMARKS OF PROF. A. D. BACHE, PRESIDENT OF THE ASSOCIATION, UPON THE MEETING OF THE AMERICAN ASSOCIATION AT CHARLESTON, S. C., MARCH, 1850.

The American Association, at its meeting at Cambridge, it will be remembered, determined to hold an intermediate meeting between the two regular annular meetings, and selected Charleston, South Carolina, as the place for holding it. The meeting was deprived of the presence of the President for the year, Professor HENRY, by the pressure of important duties, requiring immediate personal attention, and in his absence I was requested to preside; and at the close of the sessions was desired to give a notice of the doings to the members assembled at New Haven.

I had intended to give such an account as I was able of the papers read and communications made to the Charleston meeting, but the industry and zeal of the Secretary, Dr. LEWIS R. GIBBES, has, by preparing the volume of proceedings to be laid before you, prevented all necessity for such an abstract. There were thirty communications made on subjects of natural history, and eighteen on subjects of mathematical and physical science—the forty-eight papers being contributed by twenty-seven members of the Association. Many of these were of high interest, and gave rise to remarks valuable to those pursuing the subjects, and interesting to all. Some of the papers were by authors of established reputation, so that the mention of their names would be security for the value of their papers; others by men who, if life and health should be spared, will reach in future to the same point of distinction.

The germ of two most important discoveries in natural history was contained in papers by two of our youngest members. The pithy announcement by one who is the best judge of their value and correctness, has made an indelible impression upon my mind:—"The development of tissues is parallel to the growth of individuals," was the summing up by the master of a paper, which the excessive modesty of the author has forced the Secretary to enter as "not received." The contents of another were thus expressed:—"The order of succession of parts in Foraminiferæ is identical with the successive development of leaves in plants, and can be expressed by the same formulæ." Such discoveries, just warm from the study, it may be, as in these cases, forced to light by the occasion of our meetings, are among our greatest triumphs in the way of advancement.

Nearly one hundred members were added to the Association, and of

this number from seventy to eighty were in attendance. These results of the first semi-annual meeting which has been attempted, are very encouraging. There was a call for such a meeting, and in the place selected by the Association; the public and private demonstrations of interest in our prosperity, by the attendance of the citizens with their wives and daughters, and the hospitality with which we were entertained, were very gratifying. The interest increased to the last. Nor did it cease with the adjournment of the Association, but left an abiding mark of the meeting in the establishment of a museum of natural history, in connection with the medical college, to be supported in a considerable degree by the liberality of the corporate authorities of the city of Charleston. This very substantial benefit to science followed closely on the liberality of the Mayor and Council constituting the same corporation, in assuming the expenses of the meeting, and of the publication of its proceedings. Could any spirit be better than this, or more thoroughly carried out in action?

I believe that even greater benefits than these material ones were derived from this meeting, in the sacrifice of old prejudices, and, not to use too strong a term, enmities, on the altar of science, the rekindling of old and kindling of new friendships, and the general and genial glow of sympathies of head and heart.

During the consultations of members, and especially in the meetings of the Standing Committee, various matters of interest to the Association generally were discussed more or less formally, and some of these I was requested informally to present at this meeting for the consideration of the Association, and, if deemed of sufficient import, for reference to the Standing Committee.

1. Some permanent form of organization is needed. Not one which will interfere with the freedom of movement of the Association, as now constituted, nor with the changeable control and variable impulse which now characterize it, but which will serve to connect, as far as may be, one meeting with the other. The experience of one meeting, as now constituted, is not transferred to another. At the Charleston meeting, we had neither the scientific proceedings nor the business papers, neither the minutes nor the accounts, of the Cambridge meeting; and but for the presence of some few members who had been at the previous meeting, there would, at the opening in Charleston, have been no connecting links between the present and past.

The Association, not having the means of availing itself regularly, and, as part of its organization, of its experience, is kept always in the

condition of infancy, as it were—vigorous apparently, but still infancy. In the British Association, unity of purpose is secured by a general body, which directs in the recess of the local moving and changeable body. At the Charleston meeting, the idea was broached in the Standing Committee, and I was requested to communicate it to the Association here, that a body of secretaries, organized from among those who are able to attend the meetings with certainty, and who are willing to undertake the labor, to serve for a limited term of—say five years; elected yearly, and part vacating their office, in turn,—taken from the different parts of the Union, so as to render the attendance of part of them in any given place almost certain, would supply this want of a permanent organization. If with this we could unite executive duties by a permanent secretary, or the officers of some scientific institution, to conduct, in the recess of the Association, our business, other than what is local and incidental to the meetings, to make sure of the action of committees, of timely preparation for meetings, and of timely action on business of a past meeting, it would contribute essentially to our prosperity, and to the advancement of science through the action of the Association.

Do our present arrangements for publication represent the character of our meetings fully? Is there any one of us who, after a diligent, close, and interested attention throughout a meeting, does not feel disappointment when he comes to look over a volume of published proceedings? It was a striking principle, stated by some of the early members of the General Association, that the great object was to communicate from one cultivator of science to another what was doing in each department for its advancement—to hold personal communication for personal improvement. Thus, absence from the meetings was to debar from access to, or part in, the proceedings. By discussions, oral accounts of researches and experiments, and observations, by friendly counsel and encouragement, science was to be advanced through its cultivators. No attempt was to be made to influence or affect the absent. At first, warmly interested in this view, I came at last to think that so much good should be made more diffusive and more permanent; and if the principle, as stated, were rigidly adhered to, we should not be acting up to the full measure of our possible usefulness. Still, it is a real principle of action. Under it, we should publish no papers, no abstracts, no accounts of our discussions. We would encounter no responsibility beyond the Association. The opposite principle would be, to act upon the present as well as upon the absent; and, to be effective, should be reduced to system. Either all valuable papers

should be published to the full extent, or none should be so published, or a selection should be made. It should be understood that an abstract only, of any, or of all, valuable papers, would be published, and that abstracts of all would be made or should be furnished by the authors.

The discussions are frequently of as much value as the papers; and yet many of them do not appear at all, or, if appearing, are without the life that really characterizes them. It was a distinguishing feature of the Charleston meeting, that, when the branches of science interlocked, the discussions were of the most useful character; and yet all these are lost, necessarily, in the proceedings, and that by no fault of those who have so ably and zealously labored in their publication, but from want of rules and arrangement by the Association itself. To avoid irregularity, we might publish written papers, in which case we would be interfering with established societies; or we might publish only abstracts of written papers, leaving their publication in full extent to other societies. It is easy to see that there are objections to either course; but it may be well to consider whether either is not better than neither. Of the oral communications, it is our rule, that abstracts shall be furnished by the authors; but how ineffectual this rule is, is shown by the number of titles in all our published proceedings of communications with the remark, "abstract not received." The secretaries of the Sections, it appears to me, should regularly make the abstracts of both written and oral communications, and in such extent that, if no abstract is furnished by the author, this may stand as a substitute. The labor thus involved would be considerable; but, by dividing it among a considerable number, it would fall lightly upon any one. Besides, there are always those who like such labor, or, at least, are willing to encounter it for the improvement in analysis which it gives, from a desire to do good, from a sense of duty—various worthy motives. As to the discussions, are they fully presented by our present arrangements? What is wanted in regard to them generally is not a full report, such as the admirable systems of stenography afford, but a discriminating, brief, and pointed note, such as can only be given by a person who understands the subject. In any single section, what one of us could undertake to give an account of the discussions on all the subjects which come up? And if we pass from one section to another, how impossible would it be for the best informed to succeed in such an attempt?

It appears to me worth consideration, whether we may or may not make more effective arrangements for printed accounts of written and

oral communications, and of discussions, than those which we have heretofore had.

2. The meeting in sections is, perhaps, indispensable to the despatch of business. At Charleston, where we had, probably, one hundred members in attendance, it was not necessary, and we found great advantage in the discussion of subjects when the different lines of science crossed each other. Thus the apparent subsidence of the coast of South Carolina, and the temperature of the artesian wells of Alabama, brought out physical facts and reasonings of interest, while the communications of Lieut. Maury, on the currents in the Atlantic, of Prof. Le Conte, on the exudation of ice from the stems of vegetables, and others, called out a corresponding contribution from the naturalists. As these remarks are reported in but a few cases, a very important feature, and one which rendered the meeting perhaps more interesting and useful than any other single one, does not appear in the published proceedings. Is it not worth considering whether, without losing the advantage of a division into sections, by which time is, no doubt, saved, and too much popularizing of the subjects avoided, we could not, in certain cases, have the advantage of general meetings for the reading of papers? If abstracts of all communications were prepared beforehand, and forwarded to the Standing Committee, they could select such communications as it was deemed desirable to have presented in general meetings; but their labors would thereby be greatly increased, and those of the members in a similar degree. Perhaps, from the title, and by conference with the authors, the committee could determine that point in a general way. I agree with the members of the Charleston Standing Committee, that the subject is worthy of consideration and effort.

3. Is it desirable or not to increase the interest of the public in our meetings, by devoting a portion of our time to popular accounts of new subjects? The meetings of the British Association include meetings of the sections which are strictly scientific, and general meetings, at which accounts of the proceedings of the sections are given by the chairman of each, where general business is transacted, and particular subjects presented by members chosen by the General Committee for that purpose. This, though strictly belonging to the diffusion of science, may not be found to be prejudicial to its advancement. It has mixed good and evil in it. May not the good predominate?

4. The liberality of the city authorities of Charleston assumed the payment of the expenses of the meeting, including the publishing of the

proceedings. This left our treasury untouched, and, indeed, made the Association financially gainers, as well as intellectually, by the meeting. The subject of increasing the general contribution, by members absent as well as present, was talked of informally, and I was requested to suggest its discussion by the Standing Committee at the present meeting.

There was one circumstance in this meeting which gave me pain—first a shock of surprise, then unalloyed pain. It was, to see that science, which I had always considered the fast friend and natural ally of religion, was, in some of its paths, or in some turns and windings, and seeming ends of those paths, regarded with suspicion by the religious. The able address of Dr. R. W. Gibbes, of Columbia, vindicating geology from suspicion of leading to infidelity, was not written without abundant and, indeed, urgent reasons, derived from circumstances in the midst of which he is. In observing European science, one of the facts which struck me most forcibly and agreeably, was the connection in England, through her University education, between science and religion, so that some of the most eminent men were ministers of both. I thought it gave higher aims, a higher tone to science and scientific men, to be thus devoted to religion, the moral and religious man being developed at the same time with the intellectual. I believed the time had long gone by, when the study of God's works could be supposed to lead away from the revelations of his word; that the language which I heard last Sunday, from a pulpit in this city, was of common consent and acceptance—"science is no ally to skepticism." It is, nevertheless, true that a lesser wave of the same class with that which rose to overwhelm geology some twenty years since, sweeps with considerable force over the Southern portion of our Union, and requires to be stayed with judgment to subsidence, unless, by making ill-shaped and untimely obstacles, we raise it to the height and force of breaking—a dangerous experiment for those who must meet the shock.

The progress of science is towards truth. But how distant the goal! We think we discern clearly; but the medium changes, and objects assume new colors, forms, and proportions. One age teaches cycle and epicyle for truth, and makes the earth a fixed centre; another approaches nearer the truth, and shows the law which ministers the will of Him who made all things and us, to give way in its turn to some higher truth. Science is emphatically progressive. Who would be so indiscreet as to hinge his religious faith upon changeable, progressive science? A master has said: "Let no man, upon a weak conceit of sobriety, or an ill-applied moderation, think or maintain that

a man can search too far, or be too well studied, in the book of God's Word, or in the book of God's works, divinity or philosophy; but rather let men endeavor an endless progress or proficiency in both,— only let men beware that they apply both to charity, and not to swelling; to use, and not to ostentation; and again, that they do not unwisely mingle or compound these learnings together."

At a subsequent period of the meeting, the following resolutions were offered by Professor SILLIMAN, and on being put by the Secretary, were unanimously adopted.

Resolved, That this Association has heard with great pleasure and approbation the appropriate and eloquent remarks of President BACHE, regarding the meeting held at Charleston, and the general scope and objects of this body: And further, *Resolved*, that the suggestions made by him, in reference to the more permanent organization of the Association, be referred for action to the Standing Committee.

The following communications were then read:

1. CONCLUDING REMARKS BY DR. B. A. GOULD, JR., ON THE VELOCITY OF THE GALVANIC CURRENT IN THE TELEGRAPH WIRES.

2. A PLAN FOR STEREOTYPING CATALOGUES BY SEPARATE TITLES; AND FOR FORMING A GENERAL STEREOTYPED CATALOGUE OF PUBLIC LIBRARIES IN THE UNITED STATES. By CHARLES C. JEWETT, Assistant Secretary and Librarian of the Smithsonian Institution.

Everything which facilitates research promotes the progress of science. Every thorough student knows from experience the value of full, accurate, and convenient catalogues and indexes. It is on these accounts that I venture to invite attention to a project, which offers to every growing library the means of issuing, at a comparatively small expense, complete annual or biennial catalogues of its treasures; and which will enable a central establishment, like the Smithsonian Institution, to publish, at stated intervals, general catalogues of all the libraries in the country.

It may not be amiss, at the outset, to glance at the nature and extent of the difficulties, which have hitherto been encountered in attempting to meet the wants of scholars in respect to printed catalogues; and which have led, or are leading, to a common abandonment of the hope of affording such desirable guides to the literary accumulations of the larger libraries of Europe.

It is, of course, entirely practicable to publish a complete and satisfactory catalogue of a library which is stationary. But most public

libraries are constantly and rapidly increasing. This circumstance, so gratifying on every other account, is the cause of the difficulties to which I have alluded.

While the catalogue of such a collection is passing through the press, new books are received, the titles of which it is impossible, in the ordinary manner of printing, to incorporate with the body of the work. Recourse must be had to a supplement. In no other way can the acquisitions of the library be made known to the public. Ere long, perhaps, as in the Library of Congress, the number of supplements may be increased to nine. The student may thus be obliged to grope his weary way through ten catalogues, instead of one, in order to ascertain whether any book which he seeks is, or is not, in the library. He cannot be certain, even then, that the book is not in the collection, for it may have been received since the last appendix was printed.

Supplements soon become intolerable. The whole catalogue must then be re-arranged and reprinted. The expense of this process may be borne, while the library is small; but it soon becomes burdensome, and ere long insupportable, even to national establishments.

There is but one course left,—not to print at all. To this no scholar consents, except from necessity.

But to this alternative, grievous as it is, nearly all the large libraries of Europe have been reluctantly driven. More than a century has passed since the printing of the catalogue of the Royal Library at Paris was commenced. It is not yet finished. No one feels in it the interest which he would, if he could hope to have it kept up complete, when once it were brought up to a given date.

Dr. Pertz, chief librarian of the Royal Library of Berlin, declares that to print the catalogue of a large library, which is constantly increasing, is to throw away money. His opinion is founded upon the supposed impossibility of keeping up the catalogue so as continually to represent the actual possessions of the library.

The first volume of the new catalogue of printed books in the British Museum was published in 1840. It is a folio of 457 pages, and contains the letter A, complete to the end of the year 1838. The letter B has not yet appeared. Mr. Panizzi, from the first, strongly opposed the printing of any part of the catalogue till the whole, up to the prescribed limit (1838), should be completed in manuscript. Time has shown the justness of his views. The Commissioners, lately appointed by the Queen to inquire into the constitution and management of the Museum, have, in their report, expressed an opinion decidedly against

the printing of the catalogue at all, and principally on the ground that it must ever remain imperfect.

One of the witnesses (the Rt. Hon. J. W. Croker), examined before the Commissioners, thus strongly states the case with respect to printing:

"You receive, I suppose, into your Library every year, some 20,000 volumes, or something like that. Why, if you had a printed catalogue dropped down from heaven to you at this moment perfect, this day twelvemonth your 20,000 interlineations would spoil the simplicity of that catalogue; again the next year 20,000 more; and the next year 20,000 more; so that at the end of four or five years, you would have your catalogue just in the condition that your new catalogue is now (the manuscript part greater than the printed part.) With that new catalogue before your eyes, I am astonished there should be any discussion about it, for there is the experiment; the experiment has been made, and failed."

Not one European library of the first class has a complete printed catalogue in one work. The Bodleian Library issued, in 1843, a catalogue in three large volumes folio, which is generally, but erroneously, supposed to contain the titles of all the books in the collection. But all books of which special catalogues had previously been published are omitted in it. For a complete catalogue of the Bodleian Library, it is necessary to procure, not only the 3 vols. folio, 1843, but also—

1. "Books relating to British Topography, &c., bequeathed to the Bodleian Library, by Rich. Gough, Esq., 4to., Oxford, 1814."

2. "Bibliotheca celeberrima Hebræa quam collegit Dav. Oppenheimerus, 8vo. Hamburgi, 1820 [in Bibl. Bodl. illata a. 1829]."

3. "Dissertationes Academicæ quibus nuper aucta est Bibl. Bodl. fol. Oxon. 1834."

4. "Printed Books and Manuscripts bequeathed by Francis Douce, Esq., to the Bodleian Library, fol., Oxford, 1840."

Therefore, one may be obliged to search five catalogues before he can ascertain whether any particular book were in that library at the end of the year 1834.

The catalogue of 1843 is only brought down to 1835. None, therefore, of the literature of the last fifteen years, and none of the acquisitions of the library during that period, are contained in it.

A supplement is now in press, which contains the additions to the library from 1835 to 1847. When this is published, it will be possible, *by consulting six catalogues*, to ascertain whether any given book was, or was not, in the library at the close of the year 1847!

In view of these facts, it is not surprising that the Commissioners on the British Museum should come to the opinion that it is unwise to print the catalogue of that library, and should advise that nothing more be attempted than to prepare and keep up a manuscript catalogue.

But, in this opinion, the English public, who look to the end without considering the difficulties of the way, do not seem cordially to acquiesce; and it will perhaps be found necessary to print, even at the estimated cost of £40,000, and with the certainty that, almost as soon as the catalogue comes from the press, the republication of it will be as loudly demanded.

This is, surely, a disheartening state of things. It applies with equal force to catalogues of all forms, alphabetical, chronological, and classed. It has been felt and lamented by every one who has had the care of an increasing library.

In seeking a remedy for this evil, the idea occurred to me, several years ago, *to stereotype the titles separately,* and to preserve the plates, or blocks, in the alphabetical order of the titles; so as to be able readily to insert additional titles in their proper places, and then to reprint the whole catalogue. By these means, the chief cost of republication—that of composition—together with the trouble of revision and correction of the press, would, except for the new titles, be avoided. The great difficulty which had so long oppressed and discouraged the librarians of Europe, and involved the libraries in expenses so enormous, would thus be overcome.

This idea, which had occurred to me before my appointment as librarian of the Smithsonian Institution, assumed, in my mind, new importance in connection with the plan of forming a General Catalogue of American libraries.

My action in the matter was checked by the discouraging opinions of several stereotypers whom I consulted. I did not, indeed, believe the difficulties which they suggested, to be insuperable; but I was so situated, that it was impossible for me at once to institute the experiments necessary to show that they could be overcome.

In the mean time the Librarian [S. F. Haven, Esq.] and the Directors of the American Antiquarian Society having heard of the plan, opened a correspondence with me on the subject, with a view to employing it in the republication of their own catalogue. The energetic aid and ingenious suggestions of one of the directors, the Rev. E. E. Hale of Worcester, have since given a new impulse to the scheme, and have induced

me to propose its adoption by the Smithsonian Institution, earlier than I had intended.*

The suggestions of Mr. Hale, seconded by the practical skill of an ingenious electrotypist, (Mr. Wilcox, of Boston,) first established the practicability of stereotyping, or electrotyping, a catalogue in separate, movable titles.

I am able to offer for your examination two specimen pages, with the aid of which I can readily explain the several methods by which titles may be thus stereotyped. The first of these is the electrotype plate, made at the suggestion of the Rev. Mr. Hale. It is a beautiful piece of work, and it demonstrates to the conviction of the most incredulous the practicability of printing catalogues in this way.

By the ordinary electrotype process, a layer of copper, about 1-40th of an inch in thickness, is deposited upon a mould, taken from the type in wax. This plate is then mounted upon a plate of type metal, say 3-8ths of an inch thick, in order to stiffen it. The titles are next separated by means of a circular saw. For printing, these titles are mounted upon iron blocks of the size of the page.

This plan might be modified by soldering the pages of the electrotype plate upon a metallic block of the height of ordinary type, and then

* It is proper for me to state, that, in the autumn of 1847, I communicated my plans to Mr. Henry Stevens, and requested him, during his proposed visit to England, to make them known to gentlemen connected with the Library of the British Museum. In February, 1849, William Desborough Cooley, Esq., in his evidence before the Museum Commission (first published in April, 1850,) proposed a plan for stereotyping titles, similar in some respects to my own. He made no mention of any other person as the originator. In the London Athenæum of May 11, 1850, this plan is attributed to Mr. Cooley, and is dwelt upon at considerable length and with great approbation. It has since been the subject of discussion in several English periodicals. Inasmuch as two persons may, and frequently do, independently of each other, invent or discover the same thing, I have nothing to say with respect to this claim, if it be such, on the part of Mr. Cooley, further than to state, as I have done, the facts and *dates* with reference to myself, as they are known to many gentlemen in this country, showing that I had taken measures to carry my scheme into operation several years before Mr. Cooley suggested in public a scheme in some degree similar, and that my plans were known at the British Museum several months before Mr. Cooley brought the matter forward.

I should be sorry to appear unduly anxious to establish any claim to priority of invention in this case. If the scheme be a good one, it matters little who originated it. My chief concern with reference to the matter is, that the scheme which I present should not be unfavorably judged on account of any impracticability in that brought forward in England.

sawing apart the titles; or by preparing a common stereotype plate in the same way; or by casting the titles separate and of the height of type.

The other specimen page which I have to offer, is a first attempt to use for our purpose a new process of stereotyping, which promises to form an era in the art. A gentleman from Indiana, Mr. Josiah Warren, is the inventor. The material which he uses for stereotyping costs not more than three cents for an octavo page. The process is so simple, that any man of average ingenuity could learn to practise it successfully by two or three days' instruction. The cost of apparatus for carrying on the work is very small. The rapidity of execution is such, that one man could produce at least 25 octavo pages a day, all finished and ready for use. The plates give a beautiful impression. They seem as durable as common stereotpye plates; and, so far as is now known, they are not in any great degree more liable to injury.

If it bears the severe tests of practical men, this new process will have been brought forward at a most favorable period for our project; for it will be seen, by the specimen here presented, that it offers extraordinary facilities for the kind of work which we require, and it will very greatly reduce the expense.

This preliminary point,—of the practicability of stereotyping by titles,—being established, I beg leave to state the proposed *manner of employing* this method of stereotyping, in the printing of catalogues of particular libraries, and of a central catalogue of all the libraries in this country.

It is as follows:

1. The Smithsonian Institution to publish Rules for the preparation of catalogues.

2. To request other institutions intending to publish catalogues of their books, to prepare them according to these rules, with a view to their being stereotyped under the direction of the Smithsonian Institution.

3. The Smithsonian Institution to pay the whole *extra* expense of stereotyping, or such part thereof as may be agreed on.

4. The stereotyped titles to remain the property of the Smithsonian Institution.

5. Every Library uniting in this plan to have the right of using all the titles in the possession of the Smithsonian Institution, as often as desired for the printing of its own catalogue by the Institution; paying only the expense of making up the pages, of the press-work, and of distributing the titles to their proper places.

6. The Smithsonian Institution to publish, as soon as possible, and, at stated intervals, general catalogues of all libraries coming into this system.

It is believed that there is no insuperable difficulty in any part of this plan, provided that the benefits to be derived from it be such as to secure the coöperation of the various libraries in the country.

To these benefits it is necessary, therefore, to direct particular attention.

In the first place, let us consider its advantages in an economical point of view to the *first* institution adopting it.

We will suppose, for example, that the American Antiquarian Society proposes to publish a new catalogue of its library. This institution printed, in 1837, a handsome and valuable catalogue, in 562 large 8vo pages, in fine type. The composition and press-work cost, we may suppose, one dollar a page. This must all now be reprinted, in order to add, in their proper places, the titles of books received since 1837, the number of which is almost equal to that of the former catalogue. If a new catalogue be now printed, in ten or twelve years it will be necessary to reprint the whole, and this process will go on till the expense of reprinting will be quite appalling. Now, had the titles of the original catalogue been stereotyped, the catalogue, instead of costing, for the composition, 500 dollars, would have cost, for composition and stereotyping, 750 dollars, counting the extra cost of stereotyping in titles 50 per cent. above that of the composition;* but the necessity of recomposing the first part would no longer exist. Five hundred dollars would therefore be saved in the first reprint, whilst the extra expense would be only 250 dollars. Thus the net gain would be 250 dollars, minus the cost of newly making up, and imposing the old matter, which would be very inconsiderable. But there would be still further gains. It would not be thought necessary to print so large editions, if the work could be reproduced at a trifling expense. The re-arrangement, too, passes from the hands of the librarian to those of the printer; and the proof-reading has already been done, once for all.

The chance for applying this system to the first edition is indeed lost; but the same reasoning, at the present moment, applies to the second. If the whole be now stereotyped, and 200 copies struck off, the accessions for the first year may be stereotyped and printed separately, and in the second or third year, a new catalogue be issued, with the additions incorporated. In the meantime, it is probable that

* I am unable to state what would be the exact cost of stereotyping or electrotyping by titles. I assume 50 per cent. extra, as a convenient rate for calculation, though, if Mr. Warren's plan succeeds, it can be done for much less.

many of the supplemental titles would have been stereotyped for other libraries, and, thus, the cost of writing them out and of stereotyping them, be spared to the Antiquarian Society. If the cost of composition for the catalogue about to be published (containing 30,000 titles or more) be stated at 1000 dollars, for an additional sum of 500 dollars the necessity of recomposing would be forever obviated, and the great advantage secured, of being able to print every two or three years, at small expense, new editions, each complete to the time of its publication.

It is thus, I think, demonstrable, that, even for the use of a single library, this plan would be economical.

Let us next consider the advantages which the second Library adopting the plan may hope from it, in an economical point of view.

We will suppose, for the sake of example, that after the Antiquarian Society's Catalogue has been stereotyped, and the titles are placed under the care of the Smithsonian Institution, it is proposed to issue a new edition of the Catalogue of the Library of Congress. What inducements would there be for adopting this plan?

It has been the practice in this library to print a new edition of the catalogue every ten years, and to issue annual supplements. About one-fifth of the Catalogue has been printed five times; two-fifths, four times, &c. Now, from what has already been said, it will be manifest how great the saving would have been, had this plan been known and followed from the first, even if the first cost of stereotyping by titles had been twice or three times that of ordinary printing; and consequently how great would be the prospective advantage of adopting the plan, even independently of any general system.

But I now suppose, that, when the plan is adopted for the Library of Congress, the catalogue of the Antiquarian Library has already been stereotyped, and that the titles are in possession of the Smithsonian Institution for the use of other libraries entering into this general arrangement. Here is a new and very important element in the calculation.

We suppose the number of titles of the Antiquarian Library, already stereotyped, to be 30,000. Some of these titles would doubtless be the same in both collections. For all that are common to the two, the expense of the *preparation* of titles, of *printing*, and of *revision*, would be saved to the Library of Congress. It is impossible to say how much the saving would be from these sources, because these two catalogues are not uniform or complete.

If the Library of Congress were properly catalogued, it would give perhaps 70,000 titles. Of these I presume not less than 15,000 would be identical with the same number in the Antiquarian Library. this rate, more than one fifth of the labor and cost of prepa ration, and of the cost of printing, would be saved by the use of titles prepared and stereotyped by others, over and above the savings already enumerated.

After the stereotyping of the Catalogue of the Antiquarian Library and the Library of Congress, we should have perhaps 85,000 stereotyped titles. Of course, the third institution adopting the plan would be likely to find a very large number of its titles identical with those already stereotyped. The amount saved by the use of titles already stereotyped would soon (perhaps in the third library) be sufficient to counterbalance the extra expenditure for stereotyping for that library. At any rate, the extra expense would be a quantity constantly and rapidly diminishing, and it would soon (certainly after the fourth or fifth large library) vanish entirely. The Smithsonian Institution would not, therefore, be required to assume the charge of an enterprise which might involve it in great and increasing expense, but rather, and solely, to help put in operation and to guide a system which will, almost immediately, pay its own way, and will soon save enormous sums of money to our public libraries.

That the *aggregate economy* of this plan would be very great, may be seen from the following statement:

There are, in the Smithsonian Library, 15,000 pages, mostly in 8vo, of catalogues of public libraries in the United States. These contain at least 450,000 titles. But according to the best estimate which I can make from a comparison of these catalogues, there are, among them not more than 150,000 *different* titles. Two thirds, then, of the whole cost of printing these catalogues the first time might have been saved, by assuming the extra expense of stereotyping the remaining third. Much more would, as is manifest from what has been said, be saved in the successive reprints.

I have put thus prominently forward the economy to be expected from the proposed enterprise, not because this, in my estimation, is the most powerful argument in favor of it; nor because I should entirely despair of its adoption were it not advantageous in a pecuniary point of view;—but because, even if there were no other reasons for it, (provided there were none against it,) the fact of its great economy would be decisive; and because in the present poverty of our institutions of learn-

ing, and in the vast number of plans for the extension of their usefulness which present themselves for consideration, and claim approval, this might stand a much smaller chance of success if it rested entirely upon other grounds than the saving of money.

Having now, however, shown its economy when employed by a single library, and its greater economy in connection with a general system, I proceed to suggest a few among the many benefits to the cause of knowledge, which the plan promises, if generally adopted.

It can hardly be necessary for me to dwell at length upon the benefits to be expected from a *general printed Catalogue* of all books in the public libraries of America. By means of it every student in America would have the means of knowing the full extent of his resources for investigation. The places where every book could be found, would be indicated in the Catalogue. A correspondence would be kept up between the Smithsonian Library and every other library in the country. A system of exchange and of general loans might, with certain stringent conditions, be established, so that all the literary treasures of the country would be measurably accessible to every scholar. When the loan of the book would be impossible, extracts might be copied, quotations verified, and researches made through the intervention of the Smithsonian Institution, which would, in many cases, be nearly as valuable to the student as the personal examination of the book.

In connection with this topic I would add:—By law a copy of every book for which a copyright shall be secured in this country is required to be delivered to the Smithsonian Institution, and to be preserved therein. It is hoped that additional legislation on this subject will, while it lightens the burdens of publishers, secure the observance of this law, with respect to our institution, *in all cases*. If, then, the books thus obtained be all preserved, they will constitute the complete printed documentary history of America during the existence of this law. It is useless to enlarge upon the value of such a collection.

If, now, a list of these books, as they come into the library, should month by month be published in the *Smithsonian Bulletin*, and the titles be immediately stereotyped, the expense would be trifling of publishing every year a catalogue of the books copyrighted in America during the year, and to print, every five years, a general catalogue of American publications up to that limit. Thus, by the monthly bulletins, the annual lists, and the quinquennial catatlogues, chronological records of American literature would be kept.

Again, this general catalogue would enable purchasers of books for public libraries to consult judiciously for the wants of the country. So poor are we in the books which scholars need—so long, at best, must we remain in a condition of provincial dependence in literary matters—that a responsibility to the whole country rests upon the man who selects the books for any public library.

Another important benefit of this system is, that it allows us to vary the form of the catalogue, at will, from the alphabetical to the classed, and to modify the classification as we please. The titles, separately stereotyped, may change their order at command. If, for example, it were required to print a separate list of all the books in the country on the subject of *meteorology*, it would be necessary merely to check off in the general catalogue the titles to be used, and to hand it to the printer to do the rest of the work.

Another great benefit of this system would be, that it would secure *uniformity* in catalogues. A good degree of uniformity would be absolutely indispensable to the success of the plan. Entire uniformity is not to be expected. Perfection is not an attribute of the works of man. But a much higher degree of uniformity might be expected to result from this system, than could otherwise be hoped for. The rules for cataloguing must be stringent, and should meet as far as possible all difficulties of detail. Nothing, so far as it can be avoided, should be left to the individual taste or judgment of the cataloguer. He should be a man of sufficient learning, accuracy, and fidelity to apply the rules. In cases of doubt, reference should be made to the central establishment—to which the whole work should be submitted, page by page, for examination and revision. Thus we should have all our catalogues formed substantially upon one plan. Now, even if the plan adopted were that of the worst of our catalogues, if all were on the same plan, this uniformity would render catalogues, thus made, far more useful than the present chaos of irregularities. But we hope that the best possible system may be adopted.

Another general consideration in favor of this plan is, that it looks towards the accomplishment of that cherished dream of scholars, *a universal catalogue.* If the system should be successful in this country, it may eventually be so in every country of Europe. When all shall have adopted and carried out the plan, each for itself, the aggregate of the general catalogues, thus formed—few in number—will embrace the whole body of literature extant, and from them, it will be no impossible task to digest and publish a universal biblio-

graphy. How much this would promote the progress of knowledge by showing more distinctly what has been attempted and accomplished, and what yet remains to be achieved, and by thus directing the outlay of intellect aright; how much, by rebuking the rashness which rushes into authorship ignorant of what others have written, and adding to the mass of books without adding to the sum of knowledge; how much by giving confidence to the true and heroic student, who fears no labor so that it bring him to the commanding height at which he aims—the summit of learning in the branch to which he devotes himself; how much such a work would, in these and other ways, promote the great objects we have in view, is well deserving of the attention of every thoughtful mind.

In America, alone, can this system be put into immediate operation. In every large country in Europe the arrears in cataloguing, or the mass of titles accumulated, would render the first expenses of the enterprise quite startling. But here all things conspire in our favor. Our libraries are now small, and mostly repetitions one of another. But they are prosperous, and will rapidly increase. Their supporters are all desirous of having printed catalogues. A central administration is necessary. This can be accepted by the Smithsonian Institution, whose position is peculiarly favorable, and whose funds are consecrated to such purposes. The enterprise requires no great outlay of money, no gigantic effort. It may go noiselesly, but rapidly into operation. There is nothing to prevent its immediate usefulness.

I would state in conclusion, that the whole subject will soon be referred to a commission of librarians and literary men, for the advantage of their opinions and suggestions. The details for the formation and printing of the catalogue have been drawn out in full, and will be submitted to the judgment of that tribunal.

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4. On the Differences of Structure of Cells, and Corresponding Differences in their Functions. By Prof. L. Agassiz.

[*Not received.*]

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4. On the Analogy between the mode of Reproduction in Plants, and the Alternating Generation observed in some Radiata. By Prof. J. D. Dana, of New Haven.

The very remarkable fact that a polyp and a Medusa may be, in some instances, different states of one and the same species, has been well established of late by the researches of Sars, Dalyell, Steenstrupp, and others; and recent important observations have been made on the subject by Professor Agassiz. The alternations are as follows:—

1. The Medusa produces eggs;—

2. The eggs, after passing through an infusorial state, fix themselves and become polyps, like Corynæ, Tubulariæ, or Campanulariæ;—

3. The polyps produce a kind of bud that finally drops off and becomes a Medusa.

Thus the egg of a Medusa, in such cases, does not produce a Medusa, except after going through the intermediate state of a polyp.

Or, if we commence with the polyp, the series is thus:—

1. The polyp produces bulbs that become Medusæ;

2. The Medusæ produce eggs;

3. The eggs produce polyps.

This is what is called by Steenstrupp "Alternation of Generations;" and he considers the earlier generation as preparing the way for the latter. It certainly seems to be a most mysterious process:—a parent producing eggs which afford a progeny of wholly different form, (even so different that naturalists have arranged the progeny in another grand division of the Radiata); and this progeny, afterwards, by a species of budding or gemmation repeating the form of the original parent.

Yet, although seemingly so mysterious, is not this mode of development common in the vegetable kingdom? Is it not the prevalent process in the plants of our gardens and fields, with which we are all familiar?

It is well known to us, that in most plants, our trees and shrubs for example, growth from the seed brings out a bud of leaves; from this bud, after elongation, other leaf-buds are often developed, each consisting, like the first, of a number of leaves. It is an admitted fact (as may be found in treatises on Vegetable Physiology) that each of these buds is a proper plant-individual, and that those constituting a tree are as distinct and independent as the several polyps of a compound

zoöphyte; and that the tree, therefore, is as much a compound group of individuals, as the zoöphyte. In some cases the plant forms but a single leaf-bud; in others, where there is successive gemmation for a period, the number is gradually multiplied, and more or less according to the habit of the species. So among polyps, there is the simple and compound Tubularia, Campanularia, and the like.

After the plant has sufficiently matured by the production and growth of its number of leaf-buds, there is a new development—a flower-bud—consisting of the same elements as the leaf-bud, but wholly unlike it in general appearance—as much so as the Medusa is unlike the polyp. The flower-individual starts as a bulb from the leaf-individual, or the group of leaf-individuals, and is analogous in every respect to the bulbs from the Campanulariæ and allied species; and when it has fully matured, it produces, like the Medusa, ovules or seed—these seed to begin the round again of successive or alternating developments.

Thus among plants the seed produce leaf-individuals; these yield bulbs or buds becoming flower-individuals; and these produce seeds; precisely, as the egg produces polyps, the polyps bulbs that develop into Medusæ, and the Medusæ eggs.

When we follow out this subject minutely, we find the analogy completely sustained even in minor points of structure and growth. The leaf-bud consists of leaves developed in a spiral order; and in the polyp, as some species show beyond doubt, the tentacles and corresponding parts are spiral in development. The same spiral character is found in the flower, but the volutions are so close as not to be distinguished readily from circles. In the Medusæ referred to, the regularly circular form is far more neatly and perfectly developed than among the polyps—as is clearly seen in a comparison of the polyp Coryna, with the elegant Sarsia, a species of which is described and beautifully delineated in Professor Agassiz' recent memoir, published by the American Academy of Arts and Sciences at Boston. The relations in structure between plants and polyps might be further dwelt upon; but for other observations the writer would refer to his volume on Zoöphytes.

The only point in which the analogy seems to fail, is that the Medusa-bud falls off before its full development, while this is not so with plants. But it is obvious that this is unimportant in its bearing on this subject. It is a consequence of the grand difference in the mode of nutrition in the two kingdoms of nature; for the plant-bud on separation loses its only means of nutriment.

The law of alternating generations is therefore no limited principle, strange and anomalous, applying only to a few Radiata. It embraces under its scope the vegetable kingdom, and it is but another instance of identity in the laws of growth in the two great departments of life.

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5. On the Continuance of the Magnetic and Meteorological Observations at the Toronto Observatory. By Prof. E. Loomis, of New York.

Prof. Loomis read an extract from a letter of Capt. Lefroy, Director of the Magnetical and Meteorological Observatory at Toronto, in which it was stated that the period fixed upon for the continuance of the observations at Toronto would expire on the 31st of March, 1851, and desiring an expression of opinion from scientific men in the United States, whether it is desirable that the Observatory at Toronto should be longer maintained. Prof. Loomis made some statements showing the desirableness of continuing those observations for a somewhat longer period; and he also proposed that the British Government and the Directors of the Hudson Bay Company be invited to co-operate with observers in the United States in a general system of meteorological observations about to be undertaken in the United States under the direction of the Smithsonian Institution. Both subjects were referred to a Committee, to recommend to the Association such action as they might judge expedient.

This paper was referred to the Standing Committee, with instructions to prepare a resolution expressing the deep sense entertained by the Association of the importance of continuing the operations of the Observatory at Toronto.

Dr. Robert Hare presented a paper, containing a statement of certain remarks made by him at the Philadelphia meeting of 1848, an incorrect copy of which had been transferred from a newspaper into the published proceedings.

It was then voted that the corrected report be published among the proceedings of this meeting.

[*See Appendix.*]

FOURTH DAY, THURSDAY, AUGUST 22, 1850.

(*Continued.*)

GENERAL MEETING,

(*Evening Session.*)

Professor HENRY, as President of the last year, delivered the Annual Address in the College Street Church.

FOURTH DAY, THURSDAY, AUGUST 22, 1850.

SECTION OF GEOLOGY AND NATURAL HISTORY.

Professor W. R. JOHNSON was appointed to the Chair.

Professor AGASSIZ remarked that in behalf of the paper on Parasites, read yesterday, and as he knew DR. BURNETT was about to visit Europe, and would therefore have opportunities to make comparisons of our Parasitic Fauna with those of Europe, he would move that DR. BURNETT be appointed a Committee under the sanction of the Association, to make such comparisons, and report thereon at the next Annual Meeting.

The motion was seconded and carried.

1. ON THE VALUE OF THE SHELLS OF MOLLUSCA FOR THE PURPOSE OF DISTINGUISHING SPECIES AND HIGHER GROUPS. BY Prof. C. B. ADAMS.

The following remarks contain little or nothing that is new; on the contrary, their object will require allusion to very familiar facts. For this, I hope that a sufficient apology may be found in the, at least, apparent diversity of views which exist in relation to the subject that I have announced.

On the one side Conchologists *are said* to have no regard to the inhabitants of the shells which they admire; on the other hand a heterogeneous class of writers publish their own opposite opinion that shells are of no more consequence in Natural History than the fur of quadrupeds, or even the clothing or houses of men.

We cannot avoid an expression of dislike to the phrase "inhabitants of shells," as applied in science to Mollusca. There would be an obvious propriety in it, if applied to hermit crabs (Paguridæ,) since they have no organic connection with the shells which they occupy. But there is such a connection between the soft parts and the shell of a Mollusca, that neither alone constitutes an individual being, but the whole together constitutes one animal. It would be no more absurd, in scientific language, to denominate birds the inhabitants of feathers, and mammals the inhabitants of fur or wool, &c. The term, "the animal," which is frequently used for the soft parts only, is liable to the same objection, —that these parts alone do not constitute an animal. We fully admit, however, the propriety of these terms as figurative language, and would therefore by no means entirely discard them.

A writer in the *Zoölogical Journal* of London has considered the question, whether the shell or "the animal" is entitled to the name of the species, and suggests that two names are necessary! This would be like giving one name to the bones and another to the flesh of a quadruped. But whether we call a skeleton a mammoth, or apply a specific name to a shell, there is no danger of being misunderstood: we neither intend to give the shell the monopoly of its name, nor to say that the mammoth never had any "soft parts." In organic nature, individuality resides in the whole being only, and is the *specific individuality* to which the name belongs.

That Conchologists do not regard the soft parts of the animal, is partly untrue and partly excusable.

In the case of amateur collectors, who value the shells for their beauty or rarity only, it is obvious that "the animals" have no other means of gaining the attention of this class of persons, except as their shells commend them to notice. The tendency, therefore, of amateur collections, in this, as in other branches of Natural History, is favorable. Many who begin as amateurs become scientific. If they do not, we yet find them availing themselves more or less of the results of Malacology for the arrangement of their shells, and in various ways encouraging the progress of investigations in which inclination or circumstances do not permit them to engage. We protest, therefore, against the application to such collectors of any terms of disparagement, but would honor those whose hours of recreation, after the fatigues of business, are occupied with the enjoyment and study of the beauties of nature.

With the Conchologists properly so called, there is no ground for any imputation. Either by personal examination of the soft parts, or by

availing themselves of observations made by others, every possible use is made of the anatomy of the animals, while their habits and geological distribution are probably as well known as those of any invertebrated animals, unless, perhaps, insects should be excepted. If a Conchologist should in these days classify shell as univalves, bivalves, and multivalves, and separate the Testacea from the naked Mollusca, he would indicate a contempt for the animals. If he should suppose that the position in which shells were once placed for description, upside down, and sideways, is that in which locomotion is effected, it might be said that he had much to learn. So far, however, is conchology from disregarding the soft parts, that there is in some quarters even a tendency to undervalue the shell.

The study of shells is important on account of the facilities which they furnish for learning the extent of the department of Mollusca. Of the 15,000 or 20,000 species which have been described, the soft parts of only a small minority have been described, and of a very large majority the shells only have been seen by naturalists. I only allude to the value of this subject to Geology, and to the utility of a division of labor in the study of the details of Natural History.

The proposition which I intend to illustrate is this: that the value of the shells of Mollusca, as a source of distinctive characters, commencing at zero for the entire department, increases as we descend through the less comprehensive groups (not of course in a uniform ratio, but irregularly,) until we arrive at the species, which may be amply distinguished by the shells only.

That the shells are of no value for the purpose of distinguishing the department of Mollusca, is evident from the multitude of naked Mollusca.

In the first subdivision into classes, they begin to be of some value The class Brachiopoda is distinguished by two valves, one dorsal and one ventral; the Conchifera always by two lateral valves, and the Tunicata, which otherwise much resemble the Conchifera, are naked. But of the Gastropoda, some are naked and others are not, and some have shells, which can be distinguished from those of the class of Articulata only when we descend to the characters proper to genera and species. Of the Pteropods also, which some regard as entitled to the rank of a class, many are naked, but the rest have peculiar shells. While a large majority of the existing Cephalopoda are naked, the shells of the testaceous species (if we include Argonauta) agree only in characters which are common to those of the Gastropoda, although easily distin-

guished in the genera. But if, with Mr. Gray, we regard Argonauta as the shell of a Gastropod, similar to Carinaria, the testaceous species have a well marked characteristic in being polythalamous.

It is very obvious, therefore, that the shells begin to be of some value in the first subdivision into classes, since of the six classes two are wholly testaceous, and may be distinguished, not by the shells alone, but by their position on the soft parts; and three are partly testaceous, of which two may be distinguished by their shells so far as they are testaceous.

In the *orders*, we find their value not much increased. The two orders of Brachiopoda are distinguished by a single character of the shells, namely, the presence or absence of articulation between the valves. This is not the *basis* of their ordinal classification, which would be artificial if founded on one character, but it is an *index* of differences which characterize the entire animals.

The two Lamarckian orders of Conchifera are separated on account of a difference in their soft parts, namely, the number (whether one or two) of the adductor muscles, which character is clearly indicated in the shells of all the families except in the Tridacnidæ, where the approximation of the two muscles presents the appearance of a single impression. In the Mytilidæ, which have several adductor muscles, some of which are inconspicuous, the defect in the ordinal character of the soft parts is itself indicated in the shells. In Mr. Gray's list of the Genera of Recent Mollusca, five orders of Conchifera are enumerated. I have not seen them characterized, but it is obvious from their genera that ordinal characters cannot be found in the shells. The two orders of Pteropoda in Mr. Gray's list, as in several other authors, are indicated by the presence or absence of a shell, although the classification of Mollusca in that elaborate and excellent paper was designed to include all the modifications that were suggested by an examination of the soft parts of 5,000 species.

The two Lamarckian orders of Gastropoda, Gastropoda proper and Trachelipods, each contain both naked and testaceous Mollusca, but, with some important exceptions, the shells in each order may be distinguished. Mr. Gray's five orders of this class cannot be distinguished by ordinal characters of the shells.

In the three orders of Cephalopoda, one is indicated by the shells of Nautilus. The other two orders, including Argonautidæ, contain each one testaceous genus, with many naked genera; but these testaceous genera are widely different.

If we modify Mr. Gray's system by dividing the Brachiopoda into two orders, and by regarding Argonautidæ as the shell of an Octopod, we have (including the Tunicata) seventeen orders, of which two are naked, ten are wholly testaceous, and five are partly testaceous. But of the fifteen which are more or less testaceous, only five are easily separated by ordinal characters in the shells, and the others are distinguished with more or less difficulty.

In descending to the *families*, we find the value of the shells much greater than in the orders. Here I will limit my remarks to the families in Mr. Gray's list of genera, both to avoid unnecessary detail, and because they are constituted with more regard to the soft parts than those of any other classification.

In the class Cephalopoda, the two testaceous genera (Argonauta being excluded from the class,) constitute as many families, which are, therefore, amply *indicated* by their shells. In the first order of Gastropoda, Pectinibranchiata, Mr. Gray makes five families, of which three, Strombidæ, Volutidæ, and Cypræadæ, are easily distinguished by their shells; but the family Muricidæ embraces some species of Pleurotoma and of Conus, whose shells are not easily distinguished by family characters from some of the Buccinidæ. We are not aware, however, that the soft parts could be distinguished with any greater facility. This is an example of the gradation of types into each other.

The second order, Phytophaga, contains twenty-five families, of which one, Atlantidæ, is naked; Tecturidæ (=Lottiadæ) is not distinguishable by ordinal characters of the shells from Patellidæ in another order; the Littorinidæ, containing Solarium, cannot be distinguished from the Trochidæ, which contains Phillippia; and Vermitidæ and Dentalidæ so closely resemble certain Annelides, that it is doubtful whether any family characters can distinguish them. The remaining nineteen families may be distinguished by their shells with greater or less facility; the difficulties, where they exist, arising from gradation of types, not from a want of correspondence between the shells and the soft parts.

The third order, Pleurobranchiata, contains five families, with a remarkable intermingling of testaceous and of naked genera; yet the shells in each family are characterized with the greatest facility, as may be seen from the well known types of four of them, Bulla, Aplysia, Umbrella, and Carinaria, with which last Mr. Gray associates Argonauta.

The fourth order, Gymnobranchiata, contains eight families, of which

two are testaceous, the Patellidæ and Chitonidæ, which are amply distinguished by their shells, except that the former is too closely allied by its shells to Lottiadæ, as above mentioned.

The fifth order, Pulmobranchiata, consisting of the air-breathing Gastropoda, contains twelve families, of which ten are testaceous, and are easily distinguished by the shells, except that Nanina is removed from the Helicidæ and placed with the Arionidæ.

Although, in the class Conchifera, the shells do not furnish good ordinal characters for the five orders of Mr. Gray's list, yet all of the forty-two families which are distributed through these orders may be easily distinguished by their shells. Also the six families of the Brachiopoda, and those four of the Pteropoda which are testaceous, are easily distinguished by their shells.

Thus of the 95 testaceous families of Mollusca, 88, or more than 92½ per cent., are distinguishable by their shells.

For the *generic* value of the shells, we may take the results in Mr. Gray's list, which is remarkable for its multiplication of genera; 810 recent genera are recognized. Of these, 152 are naked, and 648 are testaceous. Of the latter, the shells of two genera are said by Mr. Gray to be indistinguishable from certain annelides; and of seven others it is said to be impossible to characterize them by the shells only. This list, although small, is susceptible of reduction; for M. Deshayes has pointed out the difference between the shells of Vermetus and of Serpula, and Dr. Gould has discovered generic characters in the shells of Lottia, distinguishing them from Patella. Of 648 testaceous genera, 641, therefore, may be distinguished by their shells. This amounts nearly to 99 per cent.

Although the soft parts of 5,000 species of Mollusca, including both the naked and the testaceous species, are known, we are not aware of the existence of a single testaceous *species*, which is not distinguishable by the shell with as much facility as by the soft parts. The examples are very few, indeed, if any exist, in which the species are not recognized by their shells with much greater ease than by the soft parts.

Of the 20,000 species of shells which are known, the specific value of many is more or less doubtful. It is barely possible that a knowledge of the soft parts will aid materially in determining such species. But this is not probable, since the difficulties arise from the graduation of the specific types into each other, which graduation is, therefore, likely to pervade the whole animal.

A practical illustration of the value of the shells appears in the fact

that Mr. Gray, after examining the soft parts of 5,000 species, associated Argonauta with Carinaria, notwithstanding the well known statements of Madame Power.

The general proposition which we have thus endeavored to illustrate, is only a part of a yet more general fact in organic nature—that in descending from the more to the less comprehensive groups, the characters which are proper to distinguish the groups are more and more perfectly and uniformly diffused through the parts of the entire being. We have been informed, on the best authority, that the most kindred species of fishes may be distinguished by their scales only. Species of birds may be distinguished by their feathers, or by their eggs. If we descend to individuals, nothing but observation is wanting to detect their peculiarities in each part of the entire being.

We have thus far considered the subject practically; that is, we have considered the utility of the shells of Mollusca in existing systems. A few words should be added on the principles which are involved.

The principles involved in this subject are three-fold; as they belong to the structural relations of the shell, to its functional relations, and to its embryonic history. On the latter we shall not offer any remarks.

The shells of Mollusca, having their origin in the skin, correspond, strictly, with the plumage of birds, the furs of mammals, and the scales of fishes; in general, to the covering of vertebrated animals. The correspondence is exact so far as it relates to the peculiarities of color. But the details of form are more perfectly represented by the skins of Vertebrata than by the shells of Mollusca. Yet most of the details of form in the soft parts, which are not moulded in the shells, are *indicated* by constantly associated characters.

In their functional relations, shells are skeletons. They serve for the attachment of muscles and the support of the soft parts. On this ground, therefore, we may claim for them a value similar to that of the skeletons of Vertebrata, but admit the amount of the value to be less, on account of the greater simplicity of shells.

It is, therefore, true that a collection of the shells of Mollusca corresponds to a collection of both the stuffed skins and the skeletons of Vertebrata, but is of inferior value, both for the reasons just mentioned and because their presence is not universal in their department.

It is perhaps unnecessary to add that while we believe that the shells of Mollusca are sufficient for distinguishing species, we would not regard them as only the basis of species. This would be an artificial method, although in the results it would coincide with the natural me-

thod of classification. But the shells are partly the basis of, and partly the index to, specific characters.

Prof. AGASSIZ remarked that, as valuable as such investigations are at the present time, I would like to suggest the importance of a series of investigations into the relative value of the different parts of the animal, but not a comparison, for this has long ago been done.

It is the great advantage of embryology in this respect, that we are enabled to learn the real value of characters, for the purposes of classification. There is so much with common classification that is at the discretion of the Naturalist, and which therefore has no real foundation, that it may be justly called artificial, while nature has afforded sufficient data in growth, to interpret her correctly. If we take Cuvier's classification of the Mollusca, from which Naturalists have not much deviated, we shall find that upon embryological data it cannot be carried out. This is illustrated in the cephalopods and gastropods, and Nature's own expression should be regarded above the opinions of all men. I have examined carefully the question of the value of the shell with these animals, and am satisfied that it is not much over-rated, and that it is highly expressive of the intimate nature of the animal, because it is so closely connected with its organic functions. I find that in some species, those portions of the shell which have been regarded only as ornamental in their character are of much value, because they are connected with the function of the Mantle.

Prof. SILLIMAN alluded to a work of three large volumes, published several years since, upon a single species of mollusc, by Count ———, in which the author stated his belief of the organic connection of the shell as he had first seen it deposited in crystals.

Dr. BURNETT alluded to an analogous fact, of the formation of birds' eggs, as it had been shown that the shell first existed in the shape of crystals, and these most probably inside the cells, so that the disposition of the inorganic matter was essentially connected with the organic life.

Prof. ADAMS made some few remarks, expressing his satisfaction in finding his own views in such accordance with those of Prof. Agassiz.

This was followed by some remarks by Professors AGASSIZ, SILLIMAN, and ADAMS, and Dr. BURNETT.

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2. ON ANCIENT POT-HOLES IN ROCKS. By Dr. C. T. JACKSON, Boston.

Dr. JACKSON said:

I would call the attention of the Section to the occurrence of those ancient excavations in rocks, known in New England under the name of "Pot-holes."

The manner in which these excavations are formed at the base of a water-fall, is presumed to be generally known. They are produced by the gyration of loose stones resting on the rocks immediately below the falls, the force moving them being the impelling current of falling water, acting in whirlpools. A shallow basin is first formed, which soon gathers a quantity of loose pebbles and sand, and then, by the continued rotation of the pebbles, the cavity is deepened by attrition of their mullers and sand, until it reaches a considerable depth, and appears like a huge mortar, the bottom and sides of which have been ground and polished by abrasion.

Such pot-holes are abundant in the beds of rivers, near falls of water, and may be seen at Bellows' Fall, on the Connecticut river, in Vermont, and on the Androscoggin river, at Lewiston Falls, in Maine. They run in granite at both the above named localities.

My present object is to call the attention to those pot-holes which occur where no stream of water now exists, and where the present contour of the country renders it impossible that any considerable stream should pass over the spot. It is my object to cause investigations to be made to determine how far these ancient pot-holes were connected with drift agencies, with a view to the settlement of the question, whether the drift was urged on by the action of the water or of ice, or by the conjoined influence of both ice and water.

While engaged in the geological survey of the State of New Hampshire, in 1840, '41, and '42, my attention was called to some very remarkable pot-holes which exist on the mountain ridge, in Orange, N. H., and I then examined and described these excavations. [*See Report on the Geology and Mineralogy of the State of New Hampshire.*] The pot-holes of Orange occur in a linear direction corresponding in direction with the drift scratches which abound on the recently uncovered surface of the rocks, seeming to indicate that the scratches and deeper exca-

vations owed their origin to a current having the same direction. The course of the current seems to have been from the Connecticut river towards the Merrimack Valley. From the present contour of the country, it is obvious that no stream of water could flow over the high mountain gap, and produce water-falls on the summit ridge. The pot-holes are very deep; one of them (now removed by the railroad cutting) was found, by measurement made by me, to have been eleven feet deep, and was partly filled with smooth and rounded stones.

The greatest depth of the pot-holes at Bellows' Falls is five feet, and they occur in the same kind of granite as those on the Orange summit.

It is obvious, therefore, that if the pot-holes on the mountain top were excavated by a water-fall of the same power as that at Bellows' Falls, that the current must have coursed over this ridge for more than twice the length of time that the present Connecticut river has been running.

Regarding these ancient excavations in the rocks as serving in some measure as natural chronometers, of geological value, fully equivalent to river deltas, I would invite the members of this Association to collect and present all the facts they may be able to discover of a similar character.

During the past month, in company with Mr. Francis Alger, I visited and examined some interesting pot-holes which occur on the elevated land of Sussex county, New Jersey.

In Hurdstown, N. J., we examined a pot-hole which occurs in a granite mountain, two miles north from Mount Pleasant, on the west side of Rockaway river. This excavation is on the north-east side of a steep ledge of rocks, which are water-worn, as if a current had passed over the ridge, but no drift-scratches were discoverable on any part of the ledge, but loose rounded boulders were abundant in the soil and upon the rocks, and large blocks of erratic rocks were observed in the fields and on the hill-sides.

The pot-hole is about three feet in depth, and three or four feet in diameter at the top, and it is smoothly ground on its sides and bottom. From the appearances observed, it was believed that there must have been formerly a water-fall at this place, but whether resulting from the power of a glacier or not, we are not prepared to say.

On reaching Franklin, we had an opportunity of examining another pot-hole which exists at the celebrated mine of red oxide of zinc and Franklinite. This excavation is a shallow bowl, and is in white crystalline limestone. It may be seen on the east side of the mine, at the place where mining operations are now going on.

It is probable that a great number of interesting facts relating to this subject may be collected, and it is desirable that the information should be obtained and published, for it is not long since the attention of geologists was first called to the occurrence of pot-holes on elevated land, and but small areas of rock surface have thus far been examined with a view to the discovery of similar phenomena.

This was followed by remarks from Professors Hubbard and Agassiz, and Mr. A. Osborn, of Albany.

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3. On some New Plants discovered by Col. Fremont, in California. By Dr. John Torrey.

The important services rendered to science by that distinguished traveler, Col. Fremont, are known to all who have read the reports of his hazardous journey to California. He has not only made valuable additions to the geographical knowledge of our remote possessions, but has greatly increased our acquaintance with the geology and natural history of the regions which he explored. Some of his discoveries have been noticed in the reports alluded to, but many more are still unpublished. The botanical observations and collections made in each of his expeditions were much larger than those which were brought home, many of them having been lost or destroyed in the casualties to which he was exposed.

Of the collections made in Col. Fremont's third expedition, in the years 1845–6–7, no public notice has hitherto been given, except that two or three of the new plants were briefly characterized by Dr. Gray, in order to secure the priority of their discovery. In the memoir, of which this is an abstract, I have given descriptions of ten genera of Californian plants, all of them discovered in the passes and on the sides of the Sierra Nevada, by Col. Fremont. The drawings of the accompanying plants were made by Mr. Charles Sprague, of Cambridge, who ranks among the most eminent botanical draughtsmen of our day.

The first of these new plants is a remarkable genus of the natural order Portulacaceæ. It bears a dense tuft of fleshy radical spatulate leaves, from the centre of which spring several scope-like stalks, which are from three to six inches long. From the summit of these stalks there spring, in an umbellate manner, a number of dense spreading spikes, which are at first recurved or scorpioid. The flowers are subtended by bracts. The calyx consists of two large roundish membranaceous sepals,

and the corolla of four petals, while the stamens are uniformly three in number. The structure of the pistil is like that of a majority of the order. As an expression of the estimate placed on the valuable services rendered to botany by Mr. Sprague, this new genus has been called *Spraguëa.*

The next genus is a still more remarkable one. It is a tree, nearly allied to the celebrated *Cheirostemon* of Humboldt, or Hand Tree of Mexico; but is nevertheless wholly distinct from it. Like that plant, it wants the corolla, the place of which is supplied by a very large cap-like calyx, a character that is quite an anomaly in the order Bombacea, to which both genera undoubtedly belong. The leaves are lobed like those of Cheirostemon.

Several years ago, I named a genus in honor of the distinguished traveler just mentioned; but it was shown afterwards that I was anticipated a few months by Nees, who published the same genus, under the name of *Sarcobatus*, in the appendix to Prince Maximilian's travels, a rare and costly work, which was unknown in this country until several years after my description was published. As the law of nomenclature in natural history is as just as it is inexorable, the name of Nees must be adopted instead of mine, and I have called the new Bombacean genus *Fremontia*, with the specific name of *Californica.*

Another anomalous genus, apparently Bombaceous, though deviating from the leading characters of the order still more than even Cheirostemon and Fremontia, was found on the Sierra Nevada. It is apetalous, but has a large six-cleft calyx, the divided portion of which, and a part of the tube, are deciduous. There are 10 to 14 monadelphous stamens. The ovary seems to be three-celled, with several ovules; but in the very young fruit, there is but a single cell and a solitary seed. The flowers are solitary and in pairs, at the extremity of short branches. The leaves are alternate, ovate, entire, and clothed with a stellate pubescence.

I have dedicated this fine new genus to the veteran botanist, Dr. William Darlington, whose valuable works have contributed so much for the scientific reputation of our country. The old genus given to this botanist by De Candolle having been reduced to a mere section of *Desmanthus* by Bentham, I propose to call the present plant *Darlingtonia rediviva.*

Of the great natural order Rosaceæ, there were three undescribed genera in Col. Fremont's collections, all of them shrubby. One of them belongs to the sub-order Chrysobalaneæ. It is a much branched, rigid

plant, with numerous small, thick, and wedge-shaped leaves, growing in tufts at the extremity of short branches or spurs. The flowers are terminal and solitary, without petals, but with a four-parted colored calyx. There are numerous stamens, which are inserted, or rather coalesce, with a long tube or sheath, which surrounds the ovary and style. The name *Coleogyne* alludes to the latter peculiarity.

The next genus belongs to the true Rosaceæ, and is allied to Purshia. It was first discovered by Col. Fremont; but Mr. Hartweg, an English botanist, detected it afterwards in California. Mr. Bentham, who has described all the collections of Hartweg, offered to adopt any name I wished to give this plant; but I waived the privilege, and he described it under the name of *Chamæbatia foliolosa.* It is a branching shrub, about two feet high, with finely cut leaves, and loose clusters of white flowers, resembling those of the garden raspberry.

The other Rosaceous genus is nearly related to the genus Prunus, but differs in several respects, especially in being almost destitute of a style. It has very small clusters of spatulate leaves, and small white flowers produced in short spurs. I have called it *Emplectocladus fasciculatus,* on account of the entangled branches and fasciculate leaves.

Another fine new shrub discovered by Col. Fremont, belongs to the small order of Philadelphaceæ, which, however, many botanists regard as a group of Saxifragaceæ. It differs from Philadelphus, the genus to which it is most nearly allied, in the floral envelopes being pentamerous and hexamerous, the calyx almost wholly inferior, and in the separation of the exocarp from the mesocarp. The plant is shrubby, with the usual habit of the order, and the leaves are oblong, quite entire, and narrowed into a petiole at the base. I have named this genus *Carpenteria,* in memory of my departed friend, the late excellent Prof. Carpenter, of Louisiana, who laboriously and successfully investigated the botany of his native State, but who was suddenly arrested in his career while preparing an account of his researches.

The two genera of Compositæ, which I have had drawn and engraved, have already been briefly noticed by my friend Dr. Gray, in Plantæ Fendlerianæ, that the merit of discovering them might be secured for Col. Fremont. One, the *Hymenoclea,* is allied to *Franseria.* A second species of this genus was afterwards found by Maj. Emory on the River Gila, and is noticed by me in his Report as *H. monogyra.*

The other Composita is Amphipappus, and is characterized by Dr. Gray in the *Boston Journal of Natural History.* The accompanying engraving gives a perfect representation of the plant.

The tenth and last genus which is described in my paper belongs to the sub-order Monotropeæ of Ercaceæ. It differs from *Hypolitys*, to which it is most nearly allied, in the absence of the disk; the long style; strongly 5-lobed ovary, in the form of the anthers; and, lastly, in the seeds having an appendage only at one end. As the plant is remarkably fleshy, and of a red color, I have called it *Sarcodes sanguinea*.

The memoir of which this is an abstract will be found in vol. iii. of Smithsonian Contributions.

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4. On the Decomposition of Rocks and Minerals by Water impregnated with Carbonic Acid. By Professors W. B. and R. E. Rogers, of Va.

[*Not received.*]

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5. Fossil Coniferous Wood from the Lower Devonian Strata, Lebanon, Marion Co., Ky. By Geo. C. Schaeffer, Prof. Natural Philosophy and Chemistry, Centre College, Danville, Kentucky.

My attention was drawn to a large piece of this wood in our College cabinet; it proved to be a curious form, and ascertaining that it came from the above-named place, to make the matter certain, I obtained fresh supplies and the following information from my young friend, Mr. Wm. T. McElroy. The wood is found in very large quantities between Lebanon and the foot of the "Knobs," at a distance of about three or four miles from the latter, and immediately upon the well-known "Black Slate" of the West. There can, therefore, be no doubt as to the geological position. I have other specimens, but apparently of different species, from similar localities.

Large portions are sometimes found black with carbonaceous matter, which in other cases has wholly disappeared.

The medullary rays are numerous and distinct, and the whole appearance of the wood not unlike that of beach or sugar maple. No rings of annual growth can be found. The diameter of some of the trunks must have been at least two or three feet.

This tree is distinguished from Araucaria by having three, four, or more dots side by side on each fibre, and by having the dots in patches, not uniformly distributed.

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The dots themselves seem to be peculiar; under a lower power, they seem to consist of a covering dehiscent from the centre. But by careful management and higher powers, they seem to consist of cells not unlike those forming the stomates of plants, only more in number,—about four to each dot, and different in shape.

I have no better aid in making out these details than a pair of doublets made by myself and assistant, power about 90. It is on this account that I am anxious to get the decision of those better furnished than I am.

There may be more than one species,—I send mainly from one specimen. This wood may be a species allied to Voltzia, but I have no figures by which to determine it.

On motion of Prof. AGASSIZ, this wood was referred for examination and a report, to Prof. BAILEY, of West Point.

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6. ON THE RELATION BETWEEN COLORATION AND STRUCTURE IN THE HIGHER ANIMALS. By Prof. L. AGASSIZ, of Harvard.

The color of the species of our globe is that which first strikes our eye. The terrestrial flora is green. And with algæ, it has been shown that the variety of color is due to the interception of rays; thus, they are green near shore; brown, at greater depths.

We find most flowers blue or yellow, which, combined, make green, and so the variations pass on. But in the animal kingdom no such fundamental principles exist.

My observations upon the lower marine animals show the same peculiarity as with algæ,—depending on the depth in which they live; thus, species brought up, turn green. But this is not the case with higher animals,—it is more intimately connected with organization.

In all fishes, the back is darker than the lower side. The dark, then, belongs to the higher, the lighter to the lower life. This same is applicable with birds, and thus it is that the local structure modifies the color.

This same law of color is seen in genera, and extending through species, as seen in woodpeckers and dogs. But I will take one instance—the fish Bonito: we find the dark lines spread over the side, and examining, we find they terminate in finlets.

7. THE FAMILY OF VIBRIONIA (EHRENBERG) NOT ANIMALS, BUT PLANTS. By W. J. BURNETT, M. D.

Aside from the usual difficulties always attendant upon accurate microscopical study, it appears to me that, if we consider the past condition of the optical orb, bearing on microscopy, compared with the present, every one must be struck with the results of the patient Ehrenberg. On the one hand, while it appears scarcely conceivable that, with a thirty shilling microscope, he should have arrived at views concerning the relations of certain minute objects, which look no more substantial by the working of our late superior instruments—yet, on the other hand, it cannot appear very strange, that with recently made lenses, of a perfection bordering on the very limit of all possible optical powers, one might see objects in a clearer manner, and with their relations more apparent—so that there would be the best grounds for an opinion quite different from that held in former times, although having at its head the best authority.

One certainly has much hesitation and timidity in advancing opinions dissimilar from those long held by men whose authority we reverence, and this is most assuredly increased when one touches upon doctrines held by Ehrenberg as to matters relating to microscopy.

Those persons who use the microscope for the elucidation of pathological changes, are, of all microscopists, those who see in every relation the family of Infusoria, now under consideration. Their almost constant presence, in decomposing animal solids or liquids, furnishes specimens such as you might long look for in vain in prepared infusions.

In examinations of renal excretion, I have had the best opportunities for their correct study, and to those advantages may be added, as paramount above all others, the use of a microscope rarely exceeded in any country.

The family *Vibrionia* of Ehrenberg embraces five genera, viz.: *Bacterinum, Vibrio, Spirochæta, Spirillum,* and *Spirodiscus,* including about fifteen described species.

The characteristics of the family are, that the species are composed of similar pieces, united in a chain-like form ; in other words, articulated, in this or that manner, according to the genus they represent. Beyond this no special structure of each joint has ever been perceived. Therefore, there could be but a single reason for their being placed in the animal kingdom.

This is the character of their movements, which have been thought voluntary.

Two considerations, in a discussion like this, arise. 1st. What are their structural affinities. 2d. What is the real character of their motion.

Upon examining any of the species with a high power, we see it composed of distinct articles, say in chains of two to five or six. As such, this fact affords no reason why they should not be animals, since in the entozoa a similar disposition of body exists; thus—

And neither from this alone, however similar to algæ they appear, can we say that they are plants. But if, from long-continued observation, we find that even the smallest of the species do not always exist in the form of simple chains, but that, like the ordinary forms of algæ, they branch off—then this, if we can depend at all upon analogy, must lead us to infer their compound cell-plant structure; thus—

This I have particularly observed in the genus *Spirillum*, and have watched the gradual pushing out of one cell after another, with their increase of size.

In watching their gradual growth, I have been especially led to adopt the view, that the smaller are only the younger forms of the larger species. Thus, in specimens of decomposing urine, in which I have watched these changes, the most minute forms, or the vibriones proper, first appeared. Shortly, larger forms were perceived, and at this stage you had all the gradations of size, from the smallest to the largest specimens, thus leading us justly to infer that the latter were the adult forms of the former.

Ehrenberg states, that the Vibrionia reproduce or multiply, by fissuration of previously existing forms. My own observations would go to show that, like the *Torula*, they increase by the putting forth of buds, and, aside from the most direct observation, the very fact of their branching forms shows that fissuration cannot be there the only mode of increase.

The smaller or younger forms do not, I think, contain any nucleus, although, in the more advanced forms, it becomes apparent.

Now, this is in accordance with the best views bearing on the intricate formation of cells, both animal and vegetable.* This is, that the cell-sac is but a dilated nucleus, and therefore must exist previous to its own nucleus, which appears at a more advanced age of the cell.

Considering these forms, then, as only the simplest forms of plant-cell structures, it may be asked, How shall we dispose of the five distinct genera?

In the first place, it ought here to be remarked, that, where we come to organisms as minute as these, the distinguishing characteristics of genera and species become too obscure and equivocal to have much value, and the best microscopists of the present times have long ago arrived at the conclusion, that such distinctions are an attempt at refinement of science, which will not bear the test of experience.

Beginning with the genus *Vibrio*, which composes the simplest forms, I regard these as the first appearance of the young algæ, existing then as the smallest cells arranged in a linear series. The genera *Spirillum* and *Bacterium*, composed of larger forms, and of a more fine and solid structure, represent the more advanced forms; and as all algæ, as they advance in age, tend to consolidate into mycodermous forms, losing much of their primitive cell-structure, so these two genera appear to have lost their old beaded type.

As for the two remaining genera, *Spirochoeta* and *Spirodiscus*, but little is positively known. They appear to belong scarcely to the characterized other forms of this family; and as Ehrenberg himself has expressed a doubt on the subject, one may as well omit a further notice.

Therefore, in a structural point of view, the species of this family seem to be only algæ at different stages of growth.

We next refer to the subject of their motion, which has been the chief argument for their animality. It is asserted that their movements are apparently *voluntary*, and therefore belong to sentient organisms.

I am well aware that this point calls up a no mean subject of discussion, and one which has had of late attention and investigation, so that our ideas on these points cannot long remain unchanged.

Few are aware of the obscurity overhanging the causes of motion

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* *Vide* writings of C. Nägele, papers 1 and 11, in "Reports and Papers on Botany."—*Ray. Soc.*, 1849.

of organized forms, and what is quite remarkable, we appear to have no prospect of knowing much more than the little we already have.

To be sure, an animal may move by the means of its muscles; but this, it is known, is only a single step in advance, without any explanation of the phenomenon—for it appears just as inexplicable that one organized form should move with muscles as that another should move without.

We say, that this or that infusorian moves by means of its cilia, which are distinctly seen; and if our query is somewhat benumbed by an answer like this, yet the query as to the cause of the ciliary movements comes up with a still greater force. And respecting all these matters, it appears better at once to *confess* our ignorance, than to *show* it by vague theories.

In the invisible world—and especially in the domains of cells—there are constantly occurring mysterious phenomena, which must be taken as such, and of which we know nothing, except that they seem *dependent* upon what is called the life of the cell. The most prominent of these is *simple* motion.

We see attached to simple cells, both animal and vegetable cilia, which, by their own movement, propel the cell in this or that direction, in a manner not at first to be distinguished from that of a bona-fide animal. We also see cells without cilia moving in a similar manner.

Now, because these motions have a kind of directive character, they have been called voluntary, an assertion necessarily implying a *direct act of perception* on the part of the form moved—a view which must strike every one as highly unwarrantable, considering that the very complete forms themselves are only simple cells.

According to our clearest and best ideas of Physiology, I think it must ever be insisted upon, that, because an organized form has motions of a peculiar directive and adaptive character, it by no means argues that a sentient power resides behind, or that they are animals.

The animality of the Spermatozoa has for some years been given up by the clearest observers, and the same may be affirmed of ciliated motion of various cells; and the recent widely extended observations of these days have gone to show quite distinctly, that all these peculiar motions of simple cell-structures, however voluntary to all appearances they may be, belong to the life of the cell, as a functional organ, and not to any mental power, and perhaps have their cause in the continual endosmotic and exosmotic actions of the cell-membranes.

I know of nothing more confirmatory of these conclusions, obtained from constant microscopic observation, than the behavior of these forms when subjected to certain re-agents.

It appears pretty clearly settled, that organized forms, possessed of what may be termed animality, or a separate individual entity, yield more or less to the influence of electrical and other agents which appear to act without any disorganizing process.

Thus, although many of the Rotatorial Infusoria cease their motion and appear dead from an electric shock, yet the movements of ciliated cells of Spermatozoa seem not all affected, however powerful the shock may be.

With these views, I subjected the *Vibrionia* to electrical and other agents. Electrical shocks sufficient to kill small animals had no effect upon their movements. But the application of acids and of other chemical agents which impinged on the cell-structure, soon caused them to cease.

In other words, their movements seemed to be precisely like those of the Spermatozoa, aside from the usual molecular movement or Brownian mot on.

Their cell-structure, and what may be termed their *vital* (not voluntary) motion, would, then, lead us to infer that the *Vibrionia* are algous plants, and not animals or infusoria.

This view, which, founded on continued microscopical observation, I believe to be correct, places in a clearer and more comprehensible light several phenomena of almost daily occurrence.

One in particular is, that these Vibrionia should almost invariably be found to exist in infusions and liquids that contain other algæ, and especially the common *Torula*. For I do not remember ever to have seen the *Torula*, without at the same time to have seen the Vibrionia.

We know that in or upon the tartar of the teeth different algæ are found, in the midst of which are numerous of these *Vibrionia*, the infusoria on teeth of past and present writers. Considering these as lower algæ, their presence here is easily accounted for. The same may be said of their presence in the dejections from the alimentary canal, where they coexist with numerous other forms.

Finally, that Vibrionia should be revived after many years of apparent death, as is affirmed from experiment, is quite in accordance with their plant cell-structure, while it disagrees with our clearest views of animal life.

8. ON A NEW AMERICAN SAURIAN REPTILE. BY CHARLES GIRARD.

Since the zoölogical and palæontological investigations of this century have brought to light this great truth, that the natural history of a natural group of the animal kingdom is not complete, unless that group be studied in every part of the globe where it has representatives, as well as throughout the epochs which have preceded ours, every new type must deeply attract our attention.

Hitherto the order of Saurian has appeared to have but few representatives in North America. Some genera, some species, make here the whole of its cortege. No doubt this has a philosophical bearing; but before we are able to interpret it, we have still to explore a good many miles of land, new or little known. Then we shall have several members of that group, now unknown, which will one day weigh in the balance of generalizations.

The best proof of this is found in the fact, that an artist, an amateur in science, during an attempt made two years ago to reach Oregon, brought from the plain of Platte River the reptile which makes the subject of this paper. It is an iguanian lizard, without crest, of the subfamily of Pleurodontes of Duméril and Bibron,* family of Pachyglossæ of Wagler. It cannot be referred to any of the genera hitherto established in that group, and what is more remarkable with regard to it, is a striking resemblance in its physiognomy with the genus *Proctotretus* of Mexico and Chili, although having an organization completely different. It has a nearer relationship in structure to the genus *Læmanctus* of Wiegmann,† which comprehends Mexican and Brazilian species.

I propose for it the generic name of HOLBROOKIA, and hope that American naturalists will join me in acknowledging the debt of gratitude we owe to the father of American herpetology.

HOLBROOKIA—*Girard.*

Head elliptical; depressed, without being flattened; cephalic plates small, irregular, and polygonal; occipital ones still smaller. No teeth on the palate; maxillary teeth slender, in a single row on both jaws. A fold of the skin on the breast, forming a neck-ring, the outer edge of which, being bordered with a row of larger scales, give to its margin a serrated appearance. Body depressed; covered with imbricated scales;

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* Histoire Naturelle des Reptiles, iv., 1837, p. 35.

† Herpetologia Mexicana, i., p. 46.

the inferior ones smooth, the upper ones slightly carinated. No crest of any kind. Fingers simple. Tail conical and short. Femoral pores present. No anal pores.

It thus differs from the genus *Proctotretus*, by the absence of teeth on the palate; by the presence of small occipital plates, a short tail, and femoral pores, whilst anal pores are wanting. It differs from the genus *Læmanctus* by the the presence of the femoral pores, a much shorter tail, the absence of external auditory aperture, and by the dentition, as will be seen further on.

HOLBKOOKIA MACULATA—*Girard.*

It has already been remarked how readily, at first sight, one would be tempted to place this species in the genus *Proctotretus*, judging of it merely by its general physiognomy. No doubt that it has to stand in its neighborhood with *Læmanctus*, in a natural method, as three adjoining notes in a musical scale.

Its general form is thick and short rather than elongated, a character which is especially prominent in the female. Its entire length is between three inches and a half and four inches. The tail forms the half of the length, excluding the head. This last is oval, of greater breadth than height, the summit rounded, the profile truncated. It is covered with small, irregular, and polygonal plates, larger on the middle line of the skull than above the eye, the nose, and nape. There is a supra-orbital carina, with small elongated plates, scarcely to be seen with the naked eye. The infra-orbital plates are fewer, but longer. The eyes are placed in the middle of the length of the head. The eyelids bordered by a row of pretty little pointed plates, forming a serrated edge. The nostrils are nearer the extremity of the snout than the anterior rim of the orbit. The angle of the mouth reaches the posterior rim of the eye. The upper jaw is bordered with a row of small, very elongated plates, obliquely imbricated from behind forwards. Bordering the lower jaw there are found two rows of small angular plates, the larger ones being at the angle of the mouth. A single row of small conical teeth on both jaws; those in front acute and slightly recurved; those behind stouter and erect, with a carina separating the rounded crown from the body of the tooth. The tongue is cleft on its posterior extremity, in the shape of a semilunar concavity. There are no auditory apertures to be seen externally. The tympanum is covered by scales similar to those of the neck. On both sides of the neck, immediately behind the angles of the mouth, is a fold of the skin, which vanishes in a depression under

the head. Further backwards, and on the breast, is situated that neck-ring mentioned above, ascending as high as the shoulders. The body is sub-cylindrical, the tail conical, very stout at its origin, but tapers suddenly away, to terminate in a point.

The anterior limbs are shorter and more slender than the posterior. They possess five fingers, similar in each limb, elongated, slender, terminated by a flat and recurved nail. The fingers and nails of the posterior limbs, however, are a little larger in proportion to the limbs themselves. The fourth finger is the longest, the two external ones the shortest, the third and internal both nearly equal. There are eleven femoral pores, but no anal ones.

The scales are slightly imbricated, subcarinated on the back and sides, smooth underneath. They are smaller on the neck and at the origin of the limbs than on the sides and back. Those of the tail are indistinctly verticillated. The smallest scales are below the head, on the region of the groins, and behind the vent; they are larger on the abdomen than under the tail. The fingers are scaly all over.

Mr. William Tappan, of Boston, caught this animal the 28th of June, 1848, opposite Grand Island, Platte River. He selected a male and a female, and made hastily on the spot a colored sketch of both sexes, by which we are enabled to describe the color.

The general ground is olivaceous-brown, slightly violaceous on the sides of the head. There are, on each side of the body, two rows of quite large, irregular black patches, with a band of lighter color between each row. There is an indication of a third row of these patches, less apparent in the male, where we have in return two orange-red lines, of which the upper one extends to the end of the snout, passing over the eye; the other follows the lower jaw. In advance of the eyes, these two lines are rather yellow.

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9. On the Taconic System. By T. S. Hunt, of the Geological Commission of Canada.

The results of the survey have shown, as I had the honor to state at the last Annual Meeting at Cambridge, that the Green Mountain rocks are nothing else than the rocks of the Hudson River group, with the Shawagunk conglomerates, in a metamorphic condition. The so-called Taconic rocks are a series of sandstones, slates, and limestones, found at the western base of the Green Mountains, and bounded on the west

by the Lower Silurian rocks of New York. Although similar in lithological characters to the rocks of the upper part of the Hudson River group, and the Shawagunk grits, they have been regarded by Prof. Emmons as older than the Silurian formation, and breaking up through it. This view is, however, entirely at variance with the structure of the region as deduced both from stratigraphical and geographical structure. If these supposed Taconic rocks are really an older formation, breaking up through the Silurian rocks on the East Shore of Lake Champlain, and there playing such a conspicuous part, how comes it that a few miles only to the west, this formation has entirely disappeared, and the Silurian rocks rest directly upon the formation of syenitic gneiss and limestone which is so largely developed in Northern New York, and the same absence is uniformly marked along the whole northern outcrop of this formation, a distance many hundred miles along the north shore of the St. Lawrence. Again, on the East we have an upper Silurian formation, which is traceable from the valley of the Connecticut to the Bay des Chaleurs. Along its western base, near the province line, we recognize no other than those talcose, micaceous, and magnesian rocks, with their associated deposits, which pass into the so-called Taconic rocks, further on, by a diminution of the metamorphism. If these are not the Lower Silurian rocks, it would be necessary to suppose that formation entirely absent; for even admitting a want of conformity between them and the Upper Silurian, they should, in a region thus disturbed, be somewhere exposed.

Thus to accommodate the theory of the Taconic rocks, it is necessary to suppose in this section a total absence of the Taconic rocks at the western outcrop of the Lower Silurian, and as complete a deficiency of the Lower Silurian on the eastern side of the Taconic. The two great limestone formations mentioned, are traced, characterized by their respective fossils, for a distance of 700 miles to the N. E., to the peninsula of Gaspé, with an unvarying breadth between the boundaries of the two, of about 50 miles, constituting a feature not less remarkable geographically than geologically; while, as already remarked, we have its western border reposing directly upon the syenites and crystalline limestones. Between these two great formations, both dipping S. E. we have a series which as they go N. E. gradually lose their metamorphic character, and are recognized as the rocks of the Hudson River group. Such are the facts that lead to the conclusion than between the crystalline rocks of the East side of Lake Champlain and the North shore of the St. Lawrence, on the one hand, and the Upper Silurian limestones at the

Eastern base of the Green Mountain range on the other, there are no rocks more ancient than the Silurian.

This was followed by remarks from Profs. H. D. Rogers, Hubbard, C. B. Adams, and Mr. Hunt.

At 1 o'clock P. M., the business of the Section being finished, the Section adjourned to the morning of 23d, 10 A. M.

FOURTH DAY, THURSDAY, AUGUST 23, 1850.

SECTION OF CHEMISTRY AND MINERALOGY.

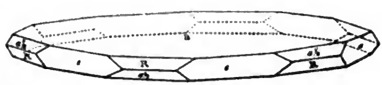
The Section met at 10 A. M., in the Lecture Room under the Trumbull Gallery. Dr. F. Bache, of Philadelphia, was called to the chair; Dr. R. E. Rogers, of Virginia, Secretary. The following communications were made.

1. Occurrence of Crystallized Oxyd of Chromium in Furnaces for manufacturing Chromate of Potash. By W. P. Blake.

Crystals of the sesquioxyd of chromium have been obtained in small quantities by Wöhler, by passing the vapor of chlorochromic acid through a tube heated to redness.

The crystals which I have examined with the following results, were obtained from a furnace which had been long in operation for the production of chromate of potassa from the mineral chromic iron. A portion of the furnace having been taken down for repairs, I found small but exceedingly brilliant crystals lining the cracks and fissures between the fire bricks, and disseminated throughout their substance. They resembled crystals of specular iron, but a blowpipe examination and qualitative analysis proved them to be the sesquioxyd of chromium.

The crystals are of the hexagonal system, and are flat 6 or 12-sided tables or scales, one of which is represented in the annexed figure. The



breadth of the largest is $1\frac{1}{2}$ lines, but they are seldom over a line broad, and usually much smaller. In some of the specimens the crystals are

grouped in rosettes of great beauty. The following angles were obtained with the reflective goniometer:

R : a = 121° 55′ (mean of 14 measurements.)

R : $a\frac{1}{2}$ = 96° 45′ (mean of 6 measurements.)

a : $a\frac{1}{2}$ = 141° 38′ 24″ (mean of 5 measurements.)

Calculating from a : R, the angle a : $a\frac{1}{2}$ = 141° 15′, and R : $a\frac{1}{2}$ = 96° 50′; and as the plane $a\frac{1}{2}$ is exceedingly small, this result is more probably correct than that given above from measurement.

The angle a : R gives for the angle of the rhombohedron 85° 22′, which is but little less than that given for specular iron. The axis = 1·39045. The crystal, according to Naumann's notation, has the descriptive expression, OR, R, $-\frac{1}{2}$R, R^{∞}.

The crystals have the hardness of sapphire, equal to 9 on the scale of Mohs. Lustre metallic. Color black; opaque except in thin plates, which are green by transmitted light. The powder of the crystals is dark green.

The specimens were taken from and between the bricks which had constituted the floor of the furnace. The furnace had been in operation for more than a year, and kept at a temperature above redness. As it needed repairs, the fires were drawn, it was allowed to cool undisturbed for ten days, and when the bricks were taken out they were still too hot to be taken in the hand.

The mass of the bricks and the portions on which the sesquioxyd has crystallized, is charged with soluble yellow chromate of potash, and in many or all of the specimens the green color of the uncrystallized oxyd can be seen.

My frequent daily examinations of the furnaces in operation made me familiar with the condition in which the contents were at different times; and considering the facts before stated, I account for the production of the crystals in the following way:

When the furnace, newly constructed or lined with fire-brick, is fired and charged with alkali and chrome ore, much of the fused chromate of potash formed is absorbed by the porous bricks, and I observed that it had penetrated through three or four courses of bricks and mortar.

After the furnace has been long in operation, the bricks become saturated, and vitrified, to a certain depth; and the floor and sides of the furnace become encrusted with a vitreous coating, which is constantly increasing. The parts more remote from the fire are consequently better protected from changes and variations of temperature, and are exempt from the inroads of more fused material.

The chromate of potash is thus kept for a long time at a uniform high temperature, and gradually losing its potash from volatilization, the chromic acid $\dddot{\mathrm{C}}\mathrm{r}$ in combination with it loses oxygen, becomes sesquioxyd $\ddot{\mathrm{C}}$ and crystallizes.

While making this communication, Mr. Blake exhibited to the Section several specimens of crystallized oxide of chromium.

Professor Silliman, Jun., and Dr. R. E. Rogers separately expressed their conviction of the interest and importance of the examination of artificial products procured under such circumstances as were calculated to throw valuable light upon the production of minerals.

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2. On a new Test for Nitrates. By Prof. G. C. Schaeffer, of Centre College, Ky., read with comments by T. S. Hunt, Canada Geological Commission.

Mr. Hunt explained Mr. Schaeffer's process to consist in adding to the solution supposed to contain nitrates, one or two drops of yellow prussiate of potash solution; these should not be enough to give a perceptible tinge to the liquid. A few drops of acetic acid are then to be added, and immediately, or in a few minutes, according to the quantity of nitrate present, the liquid assumes a yellow tint.

Dr. R. E. Rogers inquired of Mr. Hunt whether this process was designed to furnish quantitative results, expressing a doubt of its applicability for that purpose. He stated that his brother, Wm. B. Rogers, and himself, had, two years ago, in repeating the method of Jaquelain, published in the French journals, for the estimation of copper ores, by comparison of the colors of the solution, to be tested with that of standard liquids of known per centage, and hoping to apply it to other substances, especially the determination of iron in its ores, found that, with all the precautions, the eye was not delicate enough to discriminate minute differences of shade of color, to enable it to obtain accurate results.

Mr. Hunt remarked that this process was at present only designed to be a test, and not a means of quantitative analysis.

Prof. Silliman, Jr., remarked that he had obtained results which he thought satisfactory by the process of Jaquelain upon some copper ores he had examined.

3. On the assumed existence of Ammonia in the general Atmosphere. By Aug. A. Hayes, M. D., Massachusetts.

The chemistry of our atmosphere has, from the earliest time, been deemed of high interest, and its connection with the phenomena of animal and vegetable life has led to its study the minds of the most eminent chemical investigators. Of late, much significance has been given to the existence of ammonia, as a generally diffused body; and it is common in this country to hear intelligent persons speaking of this substance as a constant constituent, acting essentially in aiding the development and growth of plants, and passing into other organisms in their decay or consumption.

In connecting observations I have made on the growth of plants, my attention has been called to this subject during past years; and as the evidences on which the belief in the general distribution of ammonia is founded has not been much examined, it is my intention, at this time, to inquire into the truth of the fundamental statement, and, in so doing, to glance at the facts which chemistry supports.

Among the earliest chemical observers whose names have been preserved, we find that of the accurate and industrious Scheele. He found that saline efflorescences occurring on bottles containing acids, when decomposed, afforded volatile alkali. Sausure, also distinguished for accuracy, has left a number of observations, in which he observed this alkali absorbed from the air by acid salts, and its existence, under varied conditions. About this time, several chemists pointed out its presence in the materials of walls, in decomposing matters, and as a result of fermentations. Following these, we find the names of Chevallier, Martigny, Faraday, Zimmermann, Hermstadt, Gróger, Liebig, and many others, as authorities for the fact that ammoniacal salts and compounds exist in our atmosphere, near the surface of the earth. It is true that, in some of the cases stated, there was an obvious production of ammonia from other forms of its constituents; but both incidental observations and direct experiments establish, beyond question of doubt, an extended if not general diffusion of ammoniacal compounds near the surface of the earth, or about bodies in contact with it.

Many allusions are made to distinct sources of production, such as exist in towns, dwellings, and in decomposing masses of vegetable matter. Vegetation subject to tidal flow, as in marshes, and where spring waters mix with ocean water, are also favorable to its development.

Methods have been devised, by which the total amount of ammonia

compounds in a given mass of air can be separated, and their weight of ammonia determined. The observations of Scheele, and the direct experiments of Sausure, seem to have suggested the plans adopted. Martigny introduced the use of acids; and by a later adaptation of the apparatus of Brunner and Boussingault, permitting known volumes of air to flow through acids, so as to retain the ammonia, attempts have been made to weigh the ammonia in combination with other salts. The matter received and retained by the acid employed is concentrated with it, by heat and the ammoniacal salt, united to a heavy metallic salt, and can be weighed with great accuracy. As science progresses, we may confidently expect that the practical results obtained by new methods will carry us more surely to correct conclusions; but as the number of observations by this method increased, the striking want of accordance in the results was seen. Considerable quantities of ammonia have been found by several observers at different times; but the most trustworthy results are those of Fresenius, in a series of experiments on the air over a dwelling in Giessen. Seven millions five hundred and eleven thousand parts of air gave, as a mean quantity, *one* part of ammonia.

Another and early adopted course of experimenting is that of adding acids to water, which, as rain, has fallen through the atmosphere, or has been melted from snow, ice, and even glacier ice; by subsequent evaporation and treatment of the salts, insoluble double salts have been obtained, from the weight of which the weight of the ammonia has been deduced.

This, like the former method of research, has not added to the knowledge already possessed, and was rejected, even by the earlier chemists, on the sufficient ground that the quantity of ammonia thus found bears no fixed relation to the quantity existing. The accurate experiments of Boussingault, and other chemists, have shown that the atmosphere near the earth's surface has mixed with it, exhalations from the soil, myriads of microscopical plants, animals, and their remains. These abounding in nitrogen, in contact with acids and acid salts, *form* ammonia where it did not previously exist formed. In the conception of these methods, there has been a wide departure from chemical rules; and if the indications observed have any value for particular points of the earth's surface, they have no general significance in relation to the atmosphere.

There is another view under which we must consider the observations and experiments made for detecting or determining the amount of ammonia in the atmosphere. It is the want of recorded observations

made in the mass of the atmosphere uninfluenced by local production. The series by Fresenius, before alluded to, freed from the errors of method, would give only the proportion of ammonia over a dwelling; while the latest trials which have been made public propose to measure the quantity, where the exhalations are so abundant as to offend the senses, and act chemically on re-agents.

The exceptions thus made to the evidence of the existence of ammonia in the general atmosphere, derived from observations made on air and water from near the surface of the earth, leave us only that afforded by a more direct mode of examining rain-water which has fallen some hours after the continuance of rain. Numerous chemists have experimented on the water, which may be considered as representing the con stitution of that in the clouds falling through the lower atmosphere, previously washed from foreign matters. By slow evaporation out of contact with air, by isolating the salts by compounds without acid, or alkaline actions, or by the absorption of the water at a low temperature in confined space by substances attracting it rapidly, we can separate and detect the salts, if any, in the water.

In this way it has been found that the early hours of a rain storm afford water containing mere traces of ammonia compounds, while the later rain collected at a distance from towns and ocean, contains only occasional traces of nitrate of ammonia, and organic compounds of this base. After rain had fallen at intervals for 48 or 60 hours, no indications of ammonia were observed when the water was treated by varied methods, insuring the retention of ammonia, had any existed in the atmosphere. In the neighborhood of towns it is rare to find any rain-water, which does not contain ammoniacal salts. Water was condensed directly from the clouds, by causing the vesicles to break in contact with polished plates of metal, suspended over a point more than three thousand feet above the sea level, in a country partly covered by wood. This was carefully evaporated with moist recent chloride of silver in one case, and chloride of oxide of zinc in another, without any ammonia compounds being found.

Many distinguished chemists consider the ammonia of the atmosphere, like the chlorides and mineral matter, as accidentally present and I can find no observations recorded which contain evidence of its constant presence in the mass of the atmosphere.

The want of direct evidence establishing the truth of a statement which has been so generally believed, may lead us to place more reliance on incidental facts affording it support. Among the most

prominent, is the supposed connection between growing plants and the ammoniacal vapors assumed to exist in the general atmosphere. At least, it would be highly probable that such a distribution actually exists, could we find the slighest reason for supposing that growing vegetable organisms derive any part of their nitrogen from such a source. Now, experiments show that ammoniacal vapors are poisonous to most plants; that the salts of ammonia aid vegetation, by contributing one of two proportions of carbonic acid held in excess, or by forming soluble soil salts with acids and salts existing in the earth. If, as can be proved, elementary chlorine aids germination in a high degree when ammoniacal compounds fail, and, in vegetation, the plants appropriate nitrogen from compounds containing no ammonia, we find that we have resorted to a barren hypothesis to support a truth having a general bearing.

Nor does chemical reasoning supply any fact, in connection, supporting the received opinion. Ammonia, as such, cannot exist a moment in our atmosphere without uniting to carbonic acid, and forming one of three well known compounds. That containing the least quantity of carbonic acid rapidly passes into the salt formed from one equivalent of ammonia to two of carbonic acid. This salt is not a very volatile substance, and can rise in the atmosphere only so high as warm air extends, before it would be precipitated. In all situations where this salt spontaneously forms, we find it near the earth, and, even in warm climates, it exists as an incrustation, or imbedded deposit. The number of its sources at or near the surface of the earth is very large; but every observation correctly traced leads us to the earth as the place of its origin. All the methods which detect and separate the ammonia of the air, serve to point it out and remove it from the soil, or the immediate exhalations of the earth.

In this brief summary of the points of evidence supporting the assumed fact of the general existence of ammonia in the atmosphere, I have alluded to all that has been published on this subject, and from this we may conclude—

1st, That ammonia, in some form of combination, is always present near the earth's surface.

2d, That no experiments have been published which prove its general presence in the mass of the atmosphere.

While pursuing inquiries in this connection, I have constantly found in the atmosphere a volatile matter of complex composition, which, for some years, I supposed to be due to local exhalations; but multiplied

observations, in different and distant localities, have established its general existence. More lately, this substance has been found, by different observers in Europe and this country, coloring and contaminating the salts obtained by the evaporation of rain and snow water. Liebig notes its presence in the samples of water he operated on; but Zimmermann had earlier observed it, and, after partially separating it from other substances, named it pyrrhin.

This substance, so widely diffused, has characters which lead me to assign to it important influences; and, connected as it is with ammoniacal products, I deem its study a subject of general interest. It is to this body that we may trace the properties of rain, as distinguished from other waters.

When about 50 lbs. of recently obtained rain-water are exposed at a low temperature, in closed space, to the absorbing action of about 150 lbs. of dry caustic lime, all those substances not very volatile in an atmosphere formed in part of their own vapors, remain. Successive portions of rain-water may be thus concentrated, and the fixed parts obtained nearly dry. Generally, we find a residue of a gum-resinous appearance brown or yellow in color, and not wholly soluble in water. In the mass, are the remains of animalcula, spores of fungi, and atm - spheric dust. Water extracts other substances in mixture with pyrrhin,—mineral and ammoniacal salts, if they exist. Alcohol and ether extract parts, but evidently alter the composition of the substance. Obtained from carefully filtered solutions, pyrrhin appears as a brown yellow, adhesive substance, having a strong odor of perspired matter. Repeated evaporations render it insoluble partly, and it evidently is a changing body, having no fixed composition. In solution, its instability becomes its most marked character, and, like water which has dissolved yeast, the solution has the power of conferring motion and change of composition on other bodies. This character is displayed when it is mixed in solution with vegetable juices, weak syrups, and gum water. After its solution has been freed from ammoniacal salts, the changes following in its fermentation produce ammonia. It reduces the salts of silver and gold, blackens in concentrated sulphuric acid, forming, with this and other strong acids, salts of ammonia.

When fertile soil is undergoing fermentation, the vapors, by condensation, afford a substance much like pyrrhin. Arable soil has also in its mass a body closely connected with pyrrhin; but the state of admixture here renders it more compound than when it is obtained from the atmosphere through the aid of falling rain. The constant presence

of this body in the atmosphere, the ease with which its constituents unite to form ammonia under the presence of acids, leads to the supposition that it has been one, if not the chief source, from which experimenters have obtainted ammonia salts.

Dissolved in rain-water, and falling on the surface of the earth, this substance can induce changes under the vegetable covering which cannot result from any solution of ammoniacal salts. It has that influence which alone can impart motion or cause fermentation, and without which neither germination nor nutrition can be sustained. In New England, whatever may be the attention bestowed on preparing the soil, vegetation languishes or ceases during some weeks of the season, when the temperature of the torrid zone is exceeded, and the length of the day's heating prevents any great reduction of temperature during the night. With a dew point almost unnaturally high, there is not usually any condensation as rain, and no copious deposition as dew. Fields covered with grass become scorched, the uniform tint of ripening precedes the appearance of a dessication, which permits the winds to disperse the most of the covering, and no green color relieves the eye. If, after the grass has thus perished, and nothing but chaff-like remains can be found, rain falls for some hours, the effect is almost miraculous; the temperature of the earth near the surface does not fall; germination, more active than that of the torrid zone, succeeds, and repeatedly crops have been observed which matured on the tenth day after the rain commenced. It must be evident to every reflecting mind, that no mere salt or saline compound can give to water such power as this. Salts and their compounds cannot maintain the high temperature of the earth under the cooling effect of evaporation; and experience has, through ages of observation, shown, that those additions made to the soil, intended as the food of plants, must be in a fermenting state, or be eminently fitted to assume such conditions. The more fertile a soil, either naturally or as resulting from judicious cultivation, the more in quantity of matter, having the character of a ferment, we always find in our analysis.

It is, therefore, in view of the character which pyrrhin possesses in mixture with other bodies, of entering into a true fermentation, that I have ventured to give it so much significance as a constituent of our atmosphere, and to show that all the influence which atmospheric ammonia has been supposed to exert may, with more propriety, be attributed to pyrrhin. In a strictly chemical view, the fact that our atmosphere has an excess of carbonic acid always present, that this

excess is found in fertile soils, is opposed to any conclusion of a salt of ammonia acting in vegetation except as a carrier of carbonic acid, either directly, or by decomposition with humic compounds.

The subject was briefly discussed by Prof. HORSFORD, Dr. R. E. ROGERS, Dr. HARE, and Dr. JACKSON.

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4. ANALYSES RELATIVE TO THE ECONOMICAL VALUE OF ANTHRACITE COAL ASHES. By JONATHAN B. BUNCE, Yale Laboratory.

Coal, now the most common article of fuel in all our cities, leaves but a small quantity of ash ; yet, when we take into account the number of tons consumed in a single year, this amount becomes very considerable ; hence it becomes a matter of interest to know whether it can be considered of economical value. With this purpose in view, two samples of coal were selected, of the white and red ash varieties.

The coal was carefully burned in a furnace, the fire of which was lighted with as little charcoal as possible, and allowed first to burn several hours, in order to entirely consume the small quantity of charcoal which had been put in. The grate was then carefully and thoroughly raked out, and the ash pit cleaned, that not a trace might be left to interfere with the results.

Of the ash thus prepared, several samples were taken, to determine its solubility, both in water and acid, with the following results, viz :

White ash in water .	3·74	Red ash in water .	3·35
" acid .	17·07	" acid .	18·65

It was found that a strong acid dissolved five and six per cent. more than a weak one ; the difference consisted entirely in silica, alumina, and iron. In a qualitative analysis of the ashes, silica, alumina, iron, lime, magnesia, soda, potash, chlorine, phosphoric and sulphuric acids were detected. A quantitative analysis was then conducted, which afforded the following as a mean of two determinations, viz. :

White Ash variety.		Calculated, or soluble portion.	Red Ash variety.	Soluble portion.
Insoluble matter,	88·681		85·647	
Soluble Silica,	·091	·796	1·237	8·621
" Alumina,	3·315	35·201	4·244	29·575
Iron,	4·028	29·643	5·828	40·614
Lime,	2·111	18·655	·159	1·108

Magnesia,	·195	1·730	2·008	13·992
Soda,	·218	1·933	·164	1·146
Potash,	·162	1·443	·105	·732
Phosphoric Acid,	·198*	1·796	·269	1·880
Sulphuric "	·865	8·164	·430	3·010
Chlorine,	·087	·087	·013	·013
	99·951	99·448	100·104	100·691

The method of analysis was the same as that commonly pursued, except for the determination of the phosphoric acid, which, after many endeavors to separate from the alumina by Fresenius' method, with carbonate of baryta and caustic potash, in which alumina was invariably found in place of the phosphoric acid sought, Rose's new method, by precipitation with carb. baryta, carb. soda and alkaline semi-fusion, was then tried, which afforded satisfactory results. The alkalies were separated from magnesia by means of acetate of baryta. It will be seen, on examination, that these analyses differ mostly in the quantities of iron, lime, alumina, magnesia, and silica which they respectively contain; the lime and magnesia in particular seem to replace each other. I cannot account for the quantity of silica dissolved in one case, over the other: they were both subjected to very nearly the same heat. The results obtained in these two analyses seem to justify the extensive use of coal ashes for agricultural purposes; they are valuable both on account of the sulphates of lime and magnesia which they contain, and also from the phosphoric acid and alkalies. Sandy soils would be greatly improved from the alumina contained in them. When they are to be applied to the land, they should be carefully guarded from the influence of the weather, as much of the valuable matter would thus be washed from them. Hundreds of tons which are now wasted, might thus be brought into use. Johnston, in his work on Agricultural Chemistry, recommends highly the use of bituminous coal ashes, and cites the following analysis in support of his arguments, viz:

Scotch Bituminous Coal.

Silica and Alumina,	89·4
Lime,	3·2
Magnesia,	3·3
Iron,	1·4

* But one determination.

Sulphuric Acid,	1·7
Chlorine,	0·1
Potash and Soda,	0·3
	99·4

Our anthracite coal ashes equal these in many respects, and in some surpass them, as, although they do not contain quite as much sulphate of lime and magnesia as the specimen cited by him, the phosphoric acid in ours more than counterbalances this. Other varieties from different parts of the country ought to be analyzed, thus completing an important investigation. They would doubtless be found to vary considerably, but it is probable that a sufficient quantity of valuable substances exists in every variety, to render it of some economical importance.

Dr. R. E. ROGERS remarked that he regarded the analysis of coal ashes a subject of much interest in its bearing, and was happy to see in the results of Mr. Bunce a confirmation of a fact announced by his brother, W. B. Rogers, and himself, in the *American Journal*, more than a year ago, of the presence of alkalies in anthracite coal. Dr. R. read the following passage from their printed article:—"Among the points of interest incidentally determined during these investigations, may be mentioned the curious and instructive fact, that anthracite coal, bituminous coal, and lignite, treated by the tache process, give unequivocal evidence of alkali, while the ashes of these materials, similarly treated, yield no trace of alkali." He stated that, in their experiments, they had obtained constant and unequivocal proof that much of the alkali of these coals is volatilized and dissipated by the high temperature of their combustion.

The subject was further remarked upon by Prof. GIBBS, Prof. HORSFORD, Mr. HUNT, Mr. BLAKE, Prof. SILLIMAN, sen., Mr. WURTZ, and Prof. F. BACHE.

5. On the Relation of the Chemical Constitution of Bodies to Taste. By Prof. E. N. Horsford, of Harvard.

Prof. Horsford alluded to a paper upon Glycocoll published in 1846, in which he called attention, in a note, to the interesting relation sustained by that body in its chemical constitution, to other sweet bodies, and cited the following formulæ:

$$C_4\,H_3\overset{H}{\underset{N}{}}O_3 = \text{Glycocoll.}$$

$$C_4\,H_3\overset{H}{\underset{O}{}}O_3 + 3 = \text{Grape sugar.}$$

$$C_4\,H_3\overset{Pb}{\underset{O}{}}O_3 = \text{Sugar of lead.}$$

$$C_4\,H_3\underset{H_{\frac{2}{3}}\,O_{\frac{1}{3}}}{H}\,O_3 = \tfrac{2}{3}\ \text{Oxide of glycerile.}$$

To this enumeration of sweet bodies was appended the query, "Is this similar taste dependent upon a similar arrangement of their minutest particles?"

Prof. H. remarked that, early in 1848, he presented to the American Academy of Arts and Sciences, with a modification of the formula, an additional list of sweet bodies. Some of the formulæ were arbitrarily doubled from the received formulæ, and others fractionally reduced, for the sake, merely, of tracing this interesting relationship.

The modification of the formulæ, and the list as then presented, were as follows:

$$C_4H_4O_4 \times 3 = \text{Grape sugar.}$$

$$C_4H_2\overset{H}{\underset{O}{H}}O_3 = \text{" \quad "}$$

$$C_4H_2\overset{H}{\underset{N}{H}}O_3 = \text{Glycocoll.}$$

$$C_4H_2\overset{Pb}{\underset{O}{H}}O_3 = \text{Sugar of lead.}$$

$$C_4H_2\overset{O}{\underset{O}{Pb}}O_3\cdot 3 = \text{Citrate of lead.}$$

$$C_4H_2\overset{O}{\underset{O}{K}}O_3 = \text{Acetate of potassa.}$$

$$C_4H_2\overset{H}{\underset{N}{H}}O_3,\ H,\ O, = \text{Nitric ether.}$$

$$C_4H_2\begin{matrix}H\\H\\H_{\frac{2}{3}}O_{\frac{1}{3}}\end{matrix}\quad O_3 \times 1\cdot5 = \text{Oxide of Glycerile.}$$

$$C_4H_2\overset{Cl}{\underset{Cl}{Cl}}\,Cl_3 \div 2 = \text{Chloroform.}$$

$$C_4H_2\overset{Br}{\underset{Br}{Br}}\,Br_3 \div 2 = \text{Bromoform.*}$$

$$C_4H_2\overset{I}{\underset{I}{I}}\,I_3 \div 2 = \text{Iodoform.}$$

$$C_4H_2\overset{I}{\underset{Cl}{I}}\,Cl_3 \div 2 = \text{Chloroiodoform.}$$

$$C_4H_2\overset{Br}{\underset{I}{Br}}\,I_3 \div 2 = \text{Bromoiodoform.}$$

$$C_4H_2\begin{matrix}H\\H\\H\end{matrix}\!\begin{matrix}H\\F_2\end{matrix} \div 2 = \text{Hydrofluate of methylene.†}$$

$$C_4H_2\overset{S}{\underset{S}{S}}\,S_3 \div 2 = \text{Sulphoform.}$$

$$C_4H_2\begin{matrix}H\\H\\H\end{matrix}\!\begin{matrix}H\\Cl_2\end{matrix} \div 2 = \text{Chloride of Methyle.}$$

$$\begin{matrix}C_2\\S_2\end{matrix}H_2\overset{O}{\underset{O}{Ag}}O_3,\ H,\ O, = \text{Methylsulphite of silver.}$$

* For the observation that this and the following four bodies may be included under this type, the author acknowledged his indebtedness to his assistant Dr. Chas. H. Peirce.

† This body has a pleasant etherial smell.

$$\substack{C_2\\S_2}\,H_2\,\overset{O}{\underset{O}{Pb}},\,H,\,O, \;=\text{Methylsulphite of lead.}$$

$$\substack{C_2\\S_2}\substack{H\\Ag}\overset{O}{\underset{O}{Cl}}\,O_3,\,H,\,O=\text{Chloro-methylsulphite of silver.}$$

$$\substack{C_2\\S_2}\substack{H\\Pb}\overset{O}{\underset{O}{Cl}}\,O_3,\,H,\,O=\quad\text{"}\qquad\text{"}\qquad\text{lead.}$$

$$S_4\,Ag_2\,\overset{O}{\underset{O}{O}}\,O_3\div 2=\text{Hypo-sulphite of silver.*}$$

$$S_4\,\substack{Na\\Hg}\overset{O}{\underset{O}{O}}\,O_3=\text{Hypo-sulphite of mercury and soda}$$

To these Prof. HORSFORD remarked there might be added

$$C_4\,H_2\,\substack{H_2\\O}{}^2\,O_3\cdot 3+H_2=\text{Mannite ; and}$$

$$C_4\,H_4\,S_4.$$

Several other compounds of sulphur with $C_4\,H_4$ do not taste sweet.

The following sweet bodies, one possessing a multiple and the other nearly a multiple of the number of atoms in Glycocoll, do not readily admit of being written in the above formula—

$$C_{10}\,H_{11}\,O_3=24=\text{Valerianic acid.}$$

$$C_{16}\,H_{11}\,O_7=34=\text{Orcine.}$$

In reply to the inquiry as to whether sweetness may be ascribed to a peculiarity of form, the following facts are of significance:

$C_4\,H_4\,S_4$	tastes sweet,	and contains	C, H and S.
$C_4\,H_4\,O_4$	"	"	C, H and O.
$S_4\,\substack{O_2\\Ag_2}\,O_4$	"	"	neither C nor H.

The taste is, therefore, not dependent upon any one of the elements

* The correspondence in constitution of this body with the next in succession gives additional interest to the formula.

present, since each may be replaced entirely by another without destroying the taste.

In the communication submitted to the American Academy, attention was directed to the common formula, in which, according to Davy and to most modern chemists, all the acids containing hydrogen may be written, and also the oxygen acids which ordinarily exist in combination with an atom of water, as sulphuric and nitric acids; to wit, $H + x$; in which x represents all that part of the acid not replaced by metal in neutralization. A few examples follow:

$H + Cl$	= Hydrochloric acid.	$H + NO_6$	= Nitric acid.
$H + I$	= Hydriodic "	$H + C_2HO_4$	= Formic "
$H + Br$	= Hydrobromic "	$H + C_2O_4$	= Oxalic "
$H + F$	= Hydrofluoric "	$H + C_4H_3O_4$	= Acetic "
$H + Cy$	= Hydrocyanic "	$H + C_6H_5O_4$	= Metacetonic "
$H + SO_3$	= Sulphurous "	$H + C_8H_7O_4$	= Butyric. "
$H + SO_4$	= Sulphuric "	$H + C_{10}H_{11}O_4$	= Valerianic " &c.

The inquiry naturally arises, have sour bodies a common *form?* to which, and not to the nature of the constituent particles, the property of sourness is to be attributed. They (the acids) are composed of one larger atom, or group of atoms united to the least atom, hydrogen, easily replaceable by a metal, and bound to the group by an affinity apparently much feebler than that of any of the remaining elements.

An allusion was made, in the communication to the Academy, to the class of resins—some of the soluble members of which possessing a bitter taste—might, according to a research of Heldt upon Santonine, be referred to a single fundamental type. He refers the resins, for their origin, to the oxidation of the essential oils, and though the conception has been entertained by other chemists, it has first met with a full exposition in this paper. The hydrogen of essential oils oxidates, as a general thing, much more readily than the carbon. The following formulæ present Heldt's view of the production of resins. All are derived, he conceives, from $C_{10}H_8$.

$$[C_{10}H_8]^n - Hx + Ox = R.$$
$$[C_{10}H_8]^n - Hx + Ox + (HO)y = R.$$
$$[C_{10}H_8]^n - Hx + Ox + Oy = R.$$
$$[C_{10}H_8]^n - Hx + Ox + Oy + [HO]z = R.$$
$$[C_{10}H_8]^n + [HO]x = R.$$

These formulæ present in the original essential oils, groups of atoms, in which a part of the hydrogen occupies a more exposed situation, if one may employ the illustration, than other parts of the molecule, and oxidates more readily. In this respect there is approximation to a common form, in view of which the inquiry was suggested, *may the bitterness be ascribed to this form?* This, however, does not furnish an explanation of the remarkable bitterness of the organic bases.

Guided by the above suggestion, Prof. H. remarked, let the conception be entertained that the alkaloids have been derived from the greater or less oxidation of bodies having the general constitution of the essential oils, and the replacement of three atoms of water with one atom of ammonia for every atom of nitrogen contained in the organic base. The corresponding replacement of ammonia with three atoms of water is not of unfrequent occurrence in organic chemistry.

Upon the speculation that this has been the derivation of the alkaloids, it will be easy to convert them into the essential oils from which they were derived. If we halve the formula of the oil, the first of Heldt's formulæ gives

$$(C_5H_4)^n - Hx + Ox = R.$$

A certain number of atoms of hydrogen deducted from, and an equal number of atoms of oxygen added to, n times C_5H_4 constitute the resin. If, in adding nitrogen, we deduct for each atom of nitrogen three atoms of oxygen, or, which is the same thing, for each atom of ammonia (NH_3) added, deduct three atoms of water, we have an organic body containing C, H, N and O, a body corresponding in constitution with an alkaloid.

By reversing this process, we may convert the alkaloid into its corresponding essential oil.

Take for example Papaverin, one of the alkaloids of opium, analyzed by Merck. $C_{40}H_{21}NO_8$.*

Three atoms of oxygen for the atom of nitrogen, united to the eight atoms of oxygen, making eleven in all, correspond with eleven atoms of hydrogen, which, added to the twenty-one present, make thirty-two.

$$C_{40}H_{21}NO_8 - N - O_8 + H_8 + O_3 = C_{40}H_{29} + O_3,$$

which correspond with

$$C_{40}H_{32} = (C_5H_4)^8.$$

* Liebig's Ann., LXVI., 125; Pharm. Contr., 1848, 930.

There is doubt still resting upon the constitution of most of the alkaloids. Of those considered as best established, the following examples will be sufficient for a practical illustration of the above speculation.

Thebain $= C_{25} H_{14} NO_3 = C_{25} H_{20} = (C_5 H_4)^5$.
Furfurin $= C_{30} H_{12} N_2 O_6 = C_{30} H_{24} = (C_5 H_4)^6$.
Codein $= C_{35} H_{20} NO_5 = C_{35} H_{28} = (C_5 H_4)^7$.

The formula $(C_5 H_4)^n - H_x + O_x + O_y = R$, is suited to the derivation of Bebeerin.

$$C_{35} H_{20} NO_6 = C_{35} H_{29} = (C_5 H_4)^7 + H.$$

Harmalin requires the addition of $C_2 H_2$ to the second of Heldt's formulæ $(C_5 H_4)^n - H_x + O_y = R$.

$$C_{27} H_{14} N_2 O_2 = C_{27} H_{22} = (C_5 H_4)^5 + C_2 H_2.$$

Chinin and Cinchonin require the subtraction of C_2.

Chinin $= C_{38} H_{22} N_2 O_4$* $= C_{38} H_{32} = (C_5 H_4)^8 - C_2$.
Cinchonin $= C_{38} H_{22} N_2 O_4$† $= C_{38} H_{32} = (C_5 H_4)^8 - C_2$.

Strichnin requires the deduction of an atom of carbonic acid, or its equivalent CH_2, and the same formula of resin as that of Bebeerin.

$$C_{44} H_{24} N_2 O_4 = C_{44} H_{34} = (C_5 H_4)^9 - CH_2.$$

Cotarnin requires the addition of an atom of carbonic acid.

$$C_{26} H_{13} NO_6 \ddagger = C_{26} H_{22} = (C_5 H_4)^5 + CH_2.$$

The following require slight modifications.

Morphin $= C_{34} H_{19} NO_6$§ $= C_{34} H_{28} = (C_5 H_4)^7 - C$.
Piperin $= C_{34} H_{19} NO_6$‖ $= C_{34} H_{28} = (C_5 H_4)^7 - C$.
Veratrin $= C_{34} H_{22} NO_6 = C_{34} H_{31} = (C_5 H_4)^7 - C + H_3$.

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* Ann. Ch. Phys. [3] XIX., 363; Ann. d. Ch. u. Phar., LXII., 95.
† Ann. Ch. Phys. [3] XIX., 365.
‡ Ann. Ch. Phys. [3] XIX., 370.
§ Ann. Ch. Phys. [3] XIX., 361; Ann. d. Ch. u. Phar., LXIII., 98.
‖ Ann. Ch. Phys. [3] XIX., 363.

It will readily be seen that in this series the differences between the essential oil and its derivative are such as would disappear with the addition or subtraction of an atom of carbonic acid, or water in some cases, and a little more or less oxidation in others.

In those bases where the quantity of nitrogen is much larger, the third formula of Heldt would still give us the corresponding resins.

$$(C_5H_4)^n - Hx + Ox + Oy = R.$$

Take for example Caffein.

$$C_{14}H_8N_4O_4.$$

The nearest essential oil of the constitution $(C_5H_4)^n$ is $C_{15}H_{12}$.

$$C_{15}H_{12} - H_4 + O_4 + O_{14} + 4\,(H_3N) - 12\,HO = C_{15}H_8N_4O_6.$$

If we deduct from this formula an atom of carbonic acid, we have

$$C_{15}H_8N_4O_6 - CO_2 = C_{14}H_8N_4O_4.$$

Theobromin differs, as has been remarked,* from Caffein only by C_2H_2.

$$C_{14}H_8N_4O_4 + C_2H_2 = C_{16}H_{10}N_4O_4.$$

The construction from the class of oils, above referred to, of the formulæ of those alkaloids in which the quantity of hydrogen is much greater, is more difficult.

The foregoing relationships have an interest taken in connection with the inquiry to which attention has been directed, to wit, *May there be a common form among bodies having a common taste?*

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6. On the Analysis of Soils and the Ashes of Peat. By T. S. Hunt.

[*Not received.*]

The Section adjourned, to meet to-morrow at ten o'clock.

* Ann. d. Ch. u. Pharm., LXI., 338? Pharm. Cont. Blatt., 1847, 424.

FOURTH DAY, THURSDAY, AUGUST. 22, 1850.

SECTION OF PHYSICS AND MATHEMATICS.

Chairman—Prof. CASWELL.

Secretaries, { Prof. W. B. ROGERS, and Prof. LOOMIS.

The following communications were presented:

1. ON THE FUNDAMENTAL PRINCIPLES OF DYNAMICS, by Prof. BENJ. PEIRCE.

[*Not received.*]

Some remarks followed from Prof. HENRY, Prof. COFFIN, Mr. ALLEN, of Providence, Prof. W. B. ROGERS, and Prof. HACKLEY.

Prof. COFFIN referred to the subject of the classification and measure of power, discussed at a preceding meeting, and urged the practical difficulty of applying the same common measure to different cases, as that of common collision, the effect of a ball against a beam, and the penetration of wood or tallow.

Prof. W. B. ROGERS urged that the same measure of force was not a practice applicable to these cases. In certain modes of action, the practical measure of the force, that is, the work done, was clearly as the square of the velocity, and in others obeyed a law which was undetermined. In the former, the moving mass was always uniformly resisted, and in the other cases the resistances followed some other and remarkable law, the effects produced being incapable of exact analysis and comparison.

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2. ON LUNAR DISTANCES. By PROF. WILLIAM CHAUVENET, of the Naval Academy, Annapolis, Md.

Few problems of practical astronomy have received more attention than that of determining terrestrial longitudes by lunar distances; and though at first view it is a very simple problem indeed, most of the solutions it has received are either too prolix for the practical navigator, or *so simple at the expense of accuracy* as to be useless for the purposes to which we now wish to apply them.

Since the very general introduction of chronometers at sea, Lunar Distances are required chiefly—indeed, solely—for the purpose of testing the accurate going of the chronometers; and in order that they may really do this, it becomes necessary to revise the old methods of computation, or to give new ones.

The old methods, as I shall call them in contradistinction to Bessel's new method, all proceed from one and the same view of the problem. They rest upon the well known formula,

$$\frac{\cos d' \pm \cos (a' \pm b')}{\cos a' \cos b'} = \frac{\cos d \pm \cos (a \pm b)}{\cos a \cos b}$$

where d, a, and b are the apparent distance and altitudes, and d′, a′, b′ are the true distance and altitudes of the two bodies observed. The obvious solution of this equation is

$$\cos d' = \frac{\cos a' \cos b'}{\cos a \cos b} \left[\cos d \pm \cos (a \pm b)\right] \mp \cos (a' \pm b')$$

This gives the whole true distance d′ directly, and is the formula upon which rest certainly more than one hundred published methods, known among navigators as *rigorous* methods, and probably as many more *approximative* methods, or those in which the correction of distance is obtained.

By the mathematician or astronomer accustomed to computation this formula would be regarded as abundantly simple; but for the practical navigator it is conceived to be necessary to abridge even this short computation by the construction of certain auxiliary tables, and by the sacrifice of the rigorous character of the formula. Here, then, arises the question—to what degree of accuracy is it necessary to carry our computations at sea, considering the accuracy with which the observation of the apparent distance and altitudes can be made, and the accuracy of the lunar tables?

At first quite a rude approximation was thought sufficient, and most of the old methods are founded upon a very loose investigation of the omitted terms of the developed formula, while little or no attention was paid to atmospheric conditions, or to the corrections depending upon the height of the barometer and thermometer.

Bessel brought his scrutinizing analysis to bear upon the problem, and gave an elegant and complete solution, in which no source of error remained unexplored. He approaches the problem by a new route, altogether different from that just sketched. Considering the local time

to be always known or obtainable at sea, he dispenses with the observation of the altitudes, and makes the observation to consist simply in measuring the distance. He also dispenses with the use of the semidiameters of the bodies, the true distance of the *limbs* observed being given in the ephemeris which he proposes. Without entering here into any further explanation of this new method, I will simply remark that it leads to formulæ which, though remarkably elegant, are not simple enough for the unmathematical navigator. At least the attempt made to introduce it (about the year 1835) was unsuccessful; for although an epheremis, arranged according to Bessel's plan, was published for several years, together with auxiliary tables, by Schumacher, and although the explanations were all in the English language and (as much as was possible for a German astronomer) in the English style, the English navigators did not adopt the method, and the Americans were hardly aware of its existence.

It is now proposed by Lieut. Davis to give it another, and probably a more favorable trial, through the American Nautical Almanac; not abandoning the old methods entirely, but giving, in addition to the ordinary lunar ephemeris, another but more limited one, upon Bessel's plan, containing the distance of the moon and sun only, or, when the sun is not "in distance," of the moon and two stars, one on each side of the moon. This will give navigators a choice, and by degrees we may hope that the accurate and complete process may become better known and appreciated, from a comparison of its results with those of the old ones.

In the meantime, something may be done for the old methods; and to show how much *repair* they stand in need of, I will state that upon investigating the approximative method adopted by Lieut. Raper, of the Royal Navy, in his excellent work, (a work generally received as the best exponent of the present state of nautical astronomy), I found that the neglected terms of the formula upon which it rests may, in the cases to which Raper himself applies the method, amount to 51″ in the distance—an error which would produce nearly 30′ error in the deduced longitude!

As to the corrections for barometer and thermometer, the method just spoken of is a ready application of them, but being otherwise defective, we shall be obliged to reject it. Bowditch's methods, on the other hand, are more complete as respects the economical saving of the small terms of the formula, but are so tabulated as not to admit of barometric and thermometric corrections. As to the magnitude and

importance of these corrections, I shall simply quote Bessel's evaluation of them, viz.: that the neglect of them may produce an error of $\frac{3}{4}$ of a degree in the longitude; and cases are not uncommon in which there will be an error of $\frac{1}{3}$ or $\frac{1}{2}$ of a degree!

I may then safely assert that we now have no convenient approximative methods sufficiently accurate for the purposes of modern navigation, and at the same time sufficiently simple for the practical navigator, unacquainted, or but slightly acquainted, with mathematics. These observations will suffice to explain why I have thought it worth my while to devote some time to the investigation of this hackneyed problem, and to arrange a new and more complete method of computation.

I will not detain the Section by any discussion of my formula. I will only state that it requires special tables, which I am now engaged in constructing; that the corrections for the height of the barometer and thermometer are introduced with the greatest simplicity; and that every term of the developed formula is represented in the computation. A small term, whose maximum value is not quite 4″, is the only one which is *approximately* computed; and the approximation is within 1″, except in an extreme and improbable case, in which Venus is the body, whose distance from the moon is observed when her parallax is at its maximum, and the distance as small as 15°, and in this case the error is about 2″.

[Prof. CHAUVENET then explained the nature and use of the tables which he proposed, and particularly of a table which, in this problem, is to take the place of the ordinary refractive table.]

This communication led to remarks by Lieut. DAVIS, Prof. PEIRCE, and Prof. COAKLEY.

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3. ON GALVANIC WAVE TIME. By Prof. C. CULMANN, of Bavaria. [*See Appendix*.]

The Section then proceeded to the discussion of Prof. Mitchel's communication of Wednesday morning, "on a new method of observing and recording Astronomical R. A. & N. P. Distances."

Prof. PEIRCE made some objections to Prof. Mitchel's principle. He considered this method as involving many of the errors of the old quadrant. He compared the accuracy of Prof. M.'s observations with those of Pulkowa, and considered them decidedly inferior.

Prof. HACKLEY remarked that in Prof. M.'s instrument there was no provision against the error of eccentricity, error of graduation, &c.

Mr. S. C. WALKER said that he considered Prof. M.'s method applicable to differential observations, within a distance of a few degrees, but he doubted whether it could be applied to great distances, and alluded to the difficulty which Prof. Bessel encountered in determining the errors of the Königsberg circle.

Prof. MITCHEL replied to the criticisms of the preceding gentlemen, and concluded by moving—

That a committee of five, with Prof. Peirce for chairman, be appointed by the standing committee, to examine Prof. Mitchel's method and report at the next meeting of the Association.

This motion was adopted.

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4. NOTICE OF A POWERFUL PERMANENT MAGNET. By Prof. B. SILLIMAN, Jr.

The magnet here presented was made by M. Logeman, of Haarlem, Holland. Its weight is about ·95 of a pound, and by means of proper precautions it sustains a load of 26½ lbs., equal to more than 28 times its own weight. This weight is estimated by a common steelyard bar, and poise; the magnet being sustained with the poles uppermost in such a position that the anchor bears equally on its checks. Mr. Logeman stated that the magnet had a weight of *one* English pound, and would sustain 26½ English pounds. It appears, however, to weigh only about ·95 of a pound, and by the graduation of the steelyard arm it sustains 26½ pounds, equal as stated to over 28 times the weight of the magnet. The capacity of saturation in steel magnets of various forms has been carefully studied. Among others, by M. Hæcker, whose paper may be found in Poggendorff's *Annalen der Phys.*, &c., vol. 57, p. 335. M. Hæcker's investigations have led him to deduce a formula for the sustaining power of any magnet (n) whose mass is given, ($n=10{\cdot}33\ \sqrt[3]{n^2}$), or the sustaining power at saturation being given, the weight of the magnet can be reduced. According to M. Hæcker, the suspensive power increases as the square of the cube root of the mass, and the ratio of the suspensive power to the weight of the magnet is inversely as the cube root of the mass.

The best makers in Europe have been unable as a general thing to produce magnets sustaining more than 10 to 15 times their weight,

where this did not exceed a pound. In the present case, however, with a piece of writing paper between the checks of the magnet and the anchor, it still sustains a power of over 12 times its weight. Several sudden and successive disruptions of the anchor from this magnet do not appear to reduce its strength materially. Its power seems nearly constant.

The modes used in making and charging this magnet are not mentioned, other than that they are the result of researches by M. Elias, of Haarlem. Magnets of as great and even greater power than this are described in recent works, but they appear to have been rarely seen or constructed in this country. The case of Sir Isaac Newton's ring magnet will be remembered, which, with a weight of three grains, sustained 760 grains, or 250 times its own weight.

M. Peschel describes a mode of impregnating a horse-shoe magnet by a "feeder," in which a magnet of 1 pound sustained a weight of 26½ pounds; and M. Fischer having adopted a like mode of magnetizing the steel, has made magnets of from 1 to 3 pounds weight, that would carry 30 times their weight. The details of these processes appear not to be generally known to magneticians in this country, and now that powerful permanent magnets are of so much value in magneto-electricity, and in electro-chemical decompositions, it becomes a matter of much importance to familiarize ourselves with easy and efficient means of producing them.

M. Logeman states that he is able to furnish magnets prepared by the same process, supporting 400 and 600 pounds, at very moderate prices.

FIFTH DAY, FRIDAY, AUGUST 23, 1850.

GENERAL MEETING.

(*Morning Session.*)

The PRESIDENT in the chair.

The minutes of the meeting of Thursday afternoon were read and approved.

Prof. HORSFORD read the proceedings of the Standing Committee, stating the changes from the printed programme for the day, and reporting sundry recommendations and resolutions, whereupon it was—

Resolved, That in conformity with invitations from several learned Institutions and citizens of Albany, the next annual meeting of this Association be held in the city of Albany, to commence on the third Monday of August, 1851.

Resolved, That in conformity with the invitation of the Cincinnati Astronomical Society and citizens of Cincinnati, the intermediate meeting be held in that city, commencing on the first Monday of May, 1851.

Resolved, That Professors LOOMIS, BACHE, and HENRY be a committee to report resolutions on the subject of the continuance of the magnetic and meteorological observations of the Toronto Observatory.

Resolved, That Professor HENRY be requested to give to the Secretary a copy of his Annual Address, for publication and general distribution, and that one thousand copies be printed.

Resolved, That Messrs. JAMES D. DANA, SPENCER F. BAIRD, JEFFREYS WYMAN, JOSEPH LEIDY, and LOUIS AGASSIZ be a committee on the subject of establishing a zoological journal.

The following gentlemen were appointed the Local Committee for the meeting at Albany:—

Hon. J. V. L. PRUYN,	Prof. GEO. R. PERKINS,
" FRANKLIN TOWNSEND,	" W. W. CLARKE,
Prof. JAMES HALL,	E. P. PRENTICE, Esq.,
THOS. W. OLCOTT, Esq.,	Dr. JAS. T. MCNAUGHTON,
Dr. T. ROMEYN BECK,	" THOS. HUN,
" ALDEN MARCH,	" MASON F. COGSWELL.
" JOEL WING,	

The following gentlemen were then elected Secretaries of the Association.—

General Secretary—Prof. Wm. B. ROGERS, of Virginia.

Permanent Secretary—Prof. SPENCER F. BAIRD, of Washington City.

Local Secretary—Prof. GEORGE R. PERKINS, of Albany, N.Y.

Captain WILKES laid before the meeting, for inspection, a rollof engraved proof-sheets of plates, illustrating the botanical portion of the scientific results of the Exploring Expedition. The sheets were reserved for the Section of Geology and Natural History.

Captain WILKES, and others, offered some suggestions on th importance of permitting a free discussion of papers in the General Meetings, besides the discussions in the Section Meetings.

On motion of Professor WOLCOTT GIBBS, it was voted that a committee be appointed to present memorials to the two houses of Congress, asking that a corps of scientific observers may be attached to the Mexican Boundary Commission, and that a sum of money may be appropriated sufficient to defray the expenses of the same. The subject was then referred to the Standing Committee.

FIFTH DAY, FRIDAY, AUGUST 23, 1850.

GENERAL MEETING.

(Afternoon Session.)

The President being absent, Professor HENRY took the Chair. The minutes of the meeting in the morning were read and confirmed.

Professor HORSFORD read the proceedings of the Standing Committee in their last session.

The following gentlemen were added to the local Committee for the Albany meeting, viz:—

Professor JOHN FOSTER, Union College, Schenectady.
Prof. JONATHAN PEARSON, " " "
Professor GREEN, Rensselaer Institute, Troy.

The following gentlemen were appointed to constitute the local Committee for the Cincinnati meeting, viz:—

Hon. JACOB BURNET, Chairman.

Prof. O. M. MITCHEL.	EDWARD D. MANSFIELD,
CHARLES STETSON,	Rt. Rev. Bishop PURCELL,
Hon. TIMOTHY WALKER,	Dr. JOSEPH RAY,
Prof. HORATIO N. RÔBINSON,	JOHN P. FOOTE,
Prof. ROBERT BUCHANAN,	Dr. J. A. WARDEN,
Rev. Dr. N. L. RICE,	Hon. NATHAN GUILFORD.

Dr. THOMAS RAINEY was appointed Local Secretary for the same meeting.

The meeting was then adjourned, and the Section meetings occupied the residue of the afternoon.

FIFTH DAY, FRIDAY AUGUST 23, 1850.

GENERAL MEETING,

(*Evening Session.*)

The PRESIDENT in the chair.

The following communications were made.

ON THE WHIRLWIND THEORY OF STORMS. By DR. ROBERT HARE.

An effort to refute the whirlwind theory, or in other words to refute the opinion that in tornadoes and other travelling storms the whole of the air comprised within the theatre of stormy motion revolves about a common centre, while it is at the same time actuated by a progressive motion.

Various observations recorded by Redfield, seem to have established that certain storms, of that kind in which the winds blow in various and opposite directions, and which are consequently called hurricanes, commence in the Carribean Sea, travel first north-westerly towards the Gulf of Mexico, and then gradually bending their course into a north-easterly direction, proceed nearly parallel with the coast of the United States, percurring a distance of from fifteen hundred to two thousand miles, with a progressive velocity sometimes reaching to forty miles per hour.

I am willing to admit that there may be storms which correspond with this description. There is a great analogy between the progressive motion attributed to them, and that actually ascertained to be an attribute of tornadoes, which Mr. Redfield, justly, I believe, considers as hurricanes upon a comparatively small scale.

Nevertheless, agreeably to all that I have learned, tornadoes in the middle, northern, and eastern portions of the United States, travel from some point between north and west, to some point between south and east.

But however correct may be the allegations of Mr. Redfield respecting the existence of huricanes, of the description above given, it is certain that there are storms of another kind, to which this description is inapplicable.

Agreeably to the evidence collected by Loomis and Espy, there is a species of storm which, originating to the westward of the Mississippi,

travels from the north-west to the south-east until it goes out to sea. Storms of this description, as respects violence and blowing successively from different quarters at any one station, have all the attributes of a hurricane, and pursue a route similar to that above ascribed to tornadoes.

It is believed that, whether of the one kind or the other, hurricanes and tornadoes travel with the clouds which are instrumental in their generation. The clouds I conceive to perform the office of a coating to the electrified stratum of the atmosphere, between which and the subjacent terrestrial surface an electrical discharge taking place, causes a column of electrified air to ascend, repelled by the similarly electrified earth. Towards the area at the base of this ascending column, the air must flow in from opposite quarters, to restore the equilibrium of atmospheric density and pressure.

It is quite reasonable that the affluent currents thus produced, should gyrate, as water is seen to gyrate in a whirlpool; but in this case the gyration is a contingent consequence of the affluence of the opposite currents, and can only take place within an intermediate space towards which the opposite currents flow. As the intermediate space in the Loomis storm was not more than fifty miles in width; while in length it extended to two thousand miles, it was utterly impossible that the whole mass could revolve about a common axis. It appears that actually the winds were confined in their direction to two opposite quadrants, between north and west, and south and east.

It has been suggested to me by Prof. Henry, "that winds blowing from a higher to a lower latitude would by the earth's motion, as in the well known instance of the trades, be curved towards the west; while those blowing from the opposite quarter would be oppositely affected." To me it appears that this cause of gyration would be greater or less, or null, according to the amount of southing or northing accomplished within a given time by the winds.

The influence of this cause would lessen as the course of the storm should have in it less of northing and southing. In tornadoes the distance through which the currents move is too small for the cause in question to have a perceptible influence.

Moreover, the observations of Espy and Bache, and the observations respecting many storms collected and published by the former, are inconsistent with gyration being a usual attribute of storms.

I wish it to be understood, that I consider gyration in the atmosphere as quite consistent with laws of motion when considered as a secondary effect of centripetal currents; I only object to it when represented

as existing independently of such currents, and as extending throughout the whole area of the storm from the centre to the circumference. Evidently it is quite consistent that as a whirlpool is produced by water flowing inwards towards a descending column, so the air blowing inwards towards an ascending column should be productive of a whirlwind. But as a whirlpool can be sustained no longer than the momentum derived from the cause of influx endures, so is it with the whirlwind. *Inblowing* in the one case, is as necessary as *inflowing* in the other. Yet it would be an error to suppose that inflowing cannot take place without gyration. If any impediment be placed in the way of the whirl which may exist in a liquid running rapidly into a pipe, the whirl will be arrested, and will not be renewed although the impediment be removed. I have repeatedly realized this fact experimentally. Gyration is a contingent, not a necessary consequence of centripetal currents; unless some such cause as that suggested by HENRY, influence the result, where winds blow over the terrestrial surface more or less in a meridianal direction.

With an exhibition of much science and assiduous sagacity, ESPY has insisted that in tornadoes and travelling hurricanes the wind does not blow circuitously about a common travelling axis, but from opposite quarters towards an intermediate space or local area, over which the pressure has been diminished by the ascent of a column of air.

With ESPY'S view when restricted to tornadoes and hurricanes, I agree, *excepting as to the cause of the ascent of the air.*

It is by him assumed that this ascent arises from the expansion of the upper part of the column by the evolution of heat from condensing aqueous vapor.

Objecting to this cause as inadequate, agreeably to a reasoning elsewhere stated, I attribute the ascent of the air to a convective discharge of electricity between the earth and sky. The arguments by which I justify this view of the subject I shall postpone.

The theory which assumes the ascending column of air for its basis has been called the inblowing theory; that which supposes a general whirling in the whole aerial mass has been called the whirlwind theory.

The advocates of the latter rely upon observations obtained from the log-books of vessels at sea; but these are neutralized by the more reliable observations of ESPY, BACHE, and LOOMIS, made on terra firma. Being actuated simultaneously both by the progressive and gyratory velocities, the wind must blow in the most complicate cycloidal curves,

varying as one or the other of those velocities may be the greater, or as the observer may be more or less remote from the axis of the storm. Evidently it must be extremely difficult to show that the bearings recorded on the log-books of different vessels, are such as to harmonize with the fact that they are referable to the gyration of the same mass of air simultaneously travelling and revolving about its axis. On the other hand, it must be impossible for a navigator to infer, from the direction of a wind produced under such a complication of variable forces, his position in relation to the progressive direction of the storm, so as to know which way to escape from the violent portion as early as practicable.

Still more futile does it appear to undertake the instruction of a mariner so as to enable him to ascertain from the direction of the wind the position in which his ship may be placed relatively to the axis of any future storm, and thus to know how to avoid the more dangerous portion of the suppositious revolving aerial mass within which he may be involved. But while, from the complication of the supposed gyration with the progression of the storm, those observations which would prove it to be a whirlwind must be extremely questionable, other observations cited by the advocates of the whirlwind theory, are irreconcileable therewith; so that it may be proved erroneous by facts adduced by its friends.

According to Redfield, a storm may have any diameter between three hundred and a thousand miles in extent. At three miles from the terrestrial surface, the air is only half as dense as at that surface; we may assume three miles as the maximum of storm elevation. Imagine then a plate of air nine hundred miles wide, and three miles high, and of course having its altitude to its diameter as one to three hundred.

These proportions might be represented by an electrical plate of $\frac{1}{10}$ of an inch in thickness and 30 inches in diameter, or of $\frac{2}{10}$ of an inch in thickness and sixty inches or five feet wide, which would agree with those of the plate of a large electrical machine.

By what conceivable application of forces could such an aeriform mass be made to revolve, even if supported so as to move without friction. The glass disk, if supported without friction, would turn in obedience to any tangential force, because, in consequence of the coherence of its parts, the motion of one part involves that of the whole. Moreover, after being made to revolve, the consequent centrifugal force would be counteracted by the tenacity of its materials, but a similar impulse

would only push off a portion of the aerial plate, disturbing but little any portion which would be remote from the point of application.

To cause the whole aerial mass to revolve without derangement and so as to avoid diruption, every particle must be acted upon, at the same time, by velocities varying with the distance from the centre.

2. But the mass to be made to revolve existing as a part of the atmosphere, and there being no solid bodies in nature which can reach it, how can any cause of motion affect it without disturbing the circumambient quiescent air.

What is there to isolate it from that mass so as to induce in it separately a violent gyration?*

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* The only moving power suggested by Mr. Redfield is "*mechanical gravitation as connected with the rotary and orbitual motion of the earth.*" Admitting that these general forces could cause any portion of the atmosphere to rotate, it is inconceivable that the subjacent water of the ocean should not be similarly affected. Is it not incumbent upon the author of this idea, to show how the forces in question can be concentrated upon a particular disk of the atmosphere, so as to make it revolve within the surrounding non-revolving annulus of the same medium? "*Yet a rotative movement in the air*" is alleged by him to be "*the only cause of destructive winds and tempests.*"

For the generation of this rotative movement, the author combines with the general forces above mentioned the transient effect of obstructing bodies, as apparent in the following language.

The northern margin or parallels of the trade winds sweeping towards the Gulf, must necessarily come in collision with the archipelago of islands which skirt the Caribbean Sea. The obstruction which they afford produces a constant tendency to circular evolution.

These masses of atmosphere thus set into active revolution continue to sweep along the islands with increased rapidity of gyration, until they impinge upon the American coast.

Having thus given one of Mr. Redfield's explanations of the cause of his supposititious whirlwinds, I will proceed to give another, in which he loses sight of the planetary motions and the conflicting islands of the Caribbean Sea, and makes his whirlwinds a self-begotten progeny.

Agreeably to one of his expositions, it has been shown that every storm is due to "*a rotative movement in the air*" forgetting that as a rotative movement in the air is precisely the same thing as a whirlwind, this view of the subject makes a whirlwind a self-moving power.

The subjoined paragraph tends to confirm that view. "We observe that whirlwinds and spouts appear to commence gradually, acquire their full activity without the aid of any foreign causes; and it is well known that they are most fre-

But notwithstanding the failure of the advocates of the whirlwind theory to assign any adequate source of gyratory motion essential to them, let it be conceded that the whirling motion requisite to a hurricane has been communicated to a mass of air, of between three hundred and a thousand miles in diameter, and of an altitude not exceeding two miles,* I would inquire whether it would be possible that such a mass, while travelling as alleged at the rate of from twenty-five to forty miles per hour, could revolve about its axis when left to itself, without being soon retarded, and dissipating its momentum among surrounding portions of the atmosphere previously not participating in the whirl?

3. Is any result of the law of motion more thoroughly established, both in theory and practice, than that a whirling motion is transformed into a projectile motion in a right line, as soon as it ceases to be confined by a centripetal force, or some reaction which, like that of a string, is equivalent to a centripetal force? Is not this law verified in the case of a stone let loose from a sling, or the fragments of a fly-wheel or grindstone fractured during rapid rotation? In the case of fluids, is not the same law exemplified by the operation of the machine for winnowing grain, or of the rotary blowing machine now employed as a

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quent in those calm regions where apparently there are no currents to meet each other, and are less frequent where currents are in full activity."*

Is it conceivable that a disk or plate of air of from three hundred to a thousand miles in diameter, and less than three miles in height, may of its own accord assume a whirling motion, capable of producing the terrific violence of a hurricane?

The exclusion of all "*aid from foreign causes*" would seem to cut off all assistance from mechanical gravitation and from the rotary and orbitual motions represented as indispensable to storms, as already stated. In fact, the evidence here adduced by the author confutes his premises, which ascribe all winds and storms to planetary motion: agreeably to his own allegations, they may arise in an isolated aerial mass of the dimensions above cited, without any foreign aid, and of course without the influence of planetary motion; moreover, from the facts which he states, it must be evident that such motions can have no storm-producing influence on the isolated masses of air to which allusion has been made.

Again, the circumstances under which he represents storms to originate are such as to preclude the influence of any extraneous storm-producing agents. Their agency could not take place within a "calm region," without destroying the calm.

* It seems to me that gales do not extend higher than a mile. Mountains much less than a mile in height usually penetrate the clouds which bound the stormy stratum.

* *Silliman's Journal*, vol. xxxiii. p. 61.

bellows? What then is to prevent the air in any whirlwind, not restrained by a centripetal force, from flying off in every direction? Is there anything which can prevent its dissipation, excepting the surrounding zone of air? But as action and reaction are equal and contrary, must not this zone be pressed outward, condensed, accumulated, and agitated, with a force equal to that centrifugal force which it counteracts, so that a rapid expenditure of the gyratory momentum must ensue?

Yet this is not, as I conceive, the sole cause of expenditure. It is well known, that, agreeably to a property of fluids experimentally illustrated by Venturi,* which has been designated as their "lateral action," one portion of a fluid cannot be made to flow through another portion at rest, without incessantly dividing its force with the filaments which come in contact with it; so that the mass throughout which the momentum is thus distributed increases incessantly as it proceeds. Meanwhile the totality of the momentum receiving no addition, the velocity must lessen as the actuated mass augments. It is in consequence of this property of currents, that a jet of steam, when projected through the air, associates with it so large a quantity of the aerial particles as to excite the combustion of coal or flame. The paradoxical phenomenon of the greater adherence of two disks the more violent the effort to separate them by a blast through a tube concentric with their common axis, also the exhaustion of a syphon by blowing through an associated tube, are evidently referable to the property of air under consideration.

Agreeably to an experiment of Venturi, (*Nicholson's Journal*, quarto series, vol. ii.) by a current of water projected through a vessel, with sufficient velocity to mount over the side opposite to that at which it enters, the vessel may be partially emptied of that liquid. But if this power which a current has to associate with it other portions of the fluid in which it takes place be great, when there is no unusual pressure, how much more active must be the growth of the mass actuated, when the current is impelled against the particles at rest, by a centrifugal force? Hence, is it not evident that were tornadoes to consist of masses of air whirling on their outside border, they would grow larger in diameter, and become less and less violent the longer their duration? Yet, actually, they retain nearly the same dimensions and activity for a great part of their course, and sometimes exercise greater violence as they proceed.

* See Venturi's essay, *Nicholson's Journal*, quarto series, vol. ii.

I presume that the preceding queries can only be so answered as to sanction the inference, that an enduring rotation in a mass of air cannot take place in consequenee of its *temporary* subjection to any forces competent to make it whirl, however rapid the motion at the outset. That there are in nature any forces capable of travelling with a whirling plate of air, of the dimensions above portrayed, as assumed to exist within the precincts of whirlwind storms, so called, has never, I believe, been advanced. The existence of such forces is to me utterly inconceivable, and the evidence adduced in the case of tornadoes is irreconcilable with their existence. By the author of the theory against which I am contending, we are informed that whirlwinds and waterspouts appear to commence gradually, and to acquire their full activity without the aid of any foreign cause; and that it is well known that they are most frequent in those calm regions where, apparently, there are no active currents to meet each other, and are the least frequent where such currents abound.

But it may be urged, that as little whirlwinds are often seen, it is evident that air may be made to whirl. I have already said that no cause can be assigned for the duration of such whirls, after escaping from the stationary eddy producing obstacles by which they are generated, unless we ascribe it to a deficit of pressure about the axis of the motion, causing a centripetal force by the effort of the air to restore the equilibrium in obedience to the hydrostatic influence of gravity. I infer that whenever any eddy, producing obstacle, or any other mechanical cause, has given rise to a whirl in a fluid, there is a greater or less deficiency of pressure caused about the axis by the generating force. Hence the whirl endures for a short time after the cessation of the mechanical force to which it owes its existence, at *first directly*, and *afterwards indirectly*, in consequence of the deficit created about the axis.

Thus there are two stages in the existence of the pigmy whirlwinds in question,—1st, that in which the rotary momentum is generated and accumulated; 2nd, that in which the accumulation thus made is expended. Evidently the sum of the force to be exhausted in the latter stage can never exceed that applied in the former. Now, whatever may be the velocity with which the generating forces are applied, it must follow that there is a certain difference of level, or of pressure, at which the contripetal force arising from the conservative hydrostatic influence of gravity will become competent to counteract the disturbing centrifugal force; and it is to me clear that this equilibrium of forces must take place as soon as the whirl reaches its greatest velocity.

In fact, where a storm is travelling, as those under consideration are represented to travel by the advocates of the whirlwind theory, the gyration* being produced, as by collision with any stationary body, an island for instance, the application of the cause of gyration can be but transient, as the progression of the storm must soon cause the stationary obstacle to be left in its rear. But let it be admitted that there has been an application of forces competent to make an aerial mass revolve with any imaginable velocity, agreeably to the view above presented, the difference of level, or of density adequate to compensate the centrifugal force, cannot exceed that which would result from a single gyration with a maximum velocity. Hence the secondary force, or that which could sustain the whirl after the aerial mass actuated escapes from the influence of the cause of gyration, will not exceed the force which it has derived from that cause, which, it has already been observed, cannot go beyond the power which one whirl, at a maximum velocity, is capable of producing. Precisely in proportion to the centrifugal influence of this whirl, and consequent diminution of pressure and density within it, will be the centripetal force consequent to the hydrostatic or pneumatic effort to restore the normal equilibrium of pressure and density.

It follows that there is nothing in the phenomena of pigmy whirlwinds which can account for a greater display of gyratory force than that which may have been caused during the conflict with the deflecting body produced. It has already been advanced, that any such conflict between a mass of air, travelling from 20 to 40 miles per hour, and a stationary body, must be of very brief endurance; and that the resulting gyration, admitting it to be thus produced, must be evanescent in consequence of the rapid expenditure of the gyratory momentum.

Evidently the store of momentum created by the diminution of density or depression of level, which sustains little whirlwinds and whirlpools must be wholly inadequate to perform a similar office in the case of enormous disks of atmospheric air necessary to constitute whirlwind storms. The smallness of the altitude, in proportion to the width of the masses in questions, is a feature of great disparity, and lessens much any analogy which might exist. And were it possible that a mass of air, of which the altitude should bear the same proportion to the width, as in small whirlwinds, should extend only six miles in height, the portion occupying the upper half of the containing space having conse-

* Gyration and whirling motion are used as synonymous.

quently only one-fourth of the weight of the lower half, would create a feature of great incompatibility, from the diversity of momentum, and could not be fairly compared, as to its capability of uniform gyration, with comparatively minute masses, of which the upper and lower portions could not differ much in density.

Agreeably to the consideration above stated, I trust it will be admitted that no portion of a fluid mass, of which the parts are neither actuated by any centripetal force, nor confined by other portions of the same fluid, can revolve without being dissipated by the centrifugal force thence arising; and that if externally confined by other portions of the same fluid, the revolving portion must give off its momentum among the surrounding portions until it becomes evanescent.

The two stages of existence suggested, as respects the generation and endurance of pigmy whirlwinds, must of course arise in the case of whirlpools, originating in liquids from like causes. Those who have looked along the wake of a ship, sailing so swiftly on a tack as to require the frequent movement of the rudder, must have noticed that she leaves behind her a succession of little whirlpools. Each of these appears to originate in the following way: An occasional collision of the rudder with the water through which the ship is rapidly gliding, removes from the posterior space a portion of the fluid necessary to the level. The hydrostatic influence of gravity does not impel the water with sufficient force to enable it immediately to replenish the hiatus thus made; and there is a greater delay in the accomplishment of this result, since the liquid, being impelled towards the hiatus from every side, a conflict between the consequent centripetal currents ensues, which produces a whirl and a counteracting centrifugal force. Nevertheless, the whirl diminishes gradually in diameter, and soon ceases. It will be perceived that it is quite consistent that centripetal currents, meeting within a focal area should be productive of vortical gyration, quickening as the axis is approached.

Yet I trust it has been proved to be utterly impossible that any gyration in the whole stormy mass can display any of that violent motion immediately about the centre, which is admitted to take place in tornadoes and in hurricanes.

In addition to the objections above made to the possibility of an enduring gyratory motion being imparted to a mass of air, from three to a thousand miles in diameter, and of which the altitude cannot be more than a hundredth part of its width, its friction with the land, sea, and various rugosites and projections on the terrestrial and maritime sur-

faces, are to be considered. We can make a top spin upon its apex, but not upon its obtuse flat end. No one would undertake to make a cylindrical solid turn long on one of its bases, however smooth the supporting surface. As little would it be expected to cause it to revolve upon water durably, in obedience to any transient impulse, however forcible. It does not materially alter the case, that the superincumbent disk should be a fluid. That waves are due to the friction which takes place between wind and water, is demonstrated by the fact that they are assuaged, when, by means of oil, that friction is prevented. Hence, it is evident that whatever motion the water acquires, is at the expense of the momentum of the wind.

If an estimate were made of the momentum requisite to produce all the waves created by a hurricane in travelling from the place of its nativity in the West Indies to the Banks of Newfoundland, it would, I think, much exceed any that could be imparted to an aeriform mass of the diametrical dimension assigned to whirlwind storms by their advocates.

In any whirlwind, travelling, as admitted, from 20 to 40 miles per hour, the velocity on one side must be at least thirty miles more; and on the other at least thirty miles less than the velocity on the limbs not affected by the progressive motion. Of course, if the mean gyratory velocity be eighty miles, the whole velocity on one side would be fifty miles, on the other one hundred and ten miles. Inevitably, the violence which conflicting bodies would experience, would be as the squares of these numbers, and, consequently, the expenditure of momentum would be nearly five times as great on the south-eastern limb as on the north-western limb.

When a solid, in rotating, meets with resistance in any part whatever, the result is imparted equally to the whole; but in a fluid, especially an elastic fluid, would not an inequality of density arise, inconsistent with durable gyration? Moreover, since the upper portions of the aerial whirling mass could not be directly subjected to this cause of retardation, would not a conflict inevitably ensue between the upper and lower portions, causing a loss of the rotary power?

It must be evident, from the opinions which I have expressed, that I should never have expected that in the scientific world the hypothesis in question could have received any countenance. But the fact has been otherwise, and efforts have been made to found, on the hypothesis, practical rules for escaping from the severer parts of storms, which, agreeably to my view, could only perplex and mislead.

In this point of view it will be admitted, that the question at issue has a high practical importance, and that the more flimsy the foundation on which the whirlwind theory has been erected, the more has it been important to give it a complete refutation.

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2. Remarks upon the Care which certain Fishes take of their Eggs and Young. By Prof. Louis Agassiz.

[*Not received.*]

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3. Scientific Interest of the proposed Industrial Exhibition at London, in 1851. By Prof. Walter R. Johnson.

Professor Johnson, Secretary of the Central Committee on the Industrial Exhibition, to be held in London in 1851, made some remarks on the scientific interest of that exhibition.

He first gave a short account of the manner in which that exhibition had been presented to the American people, and stated the steps which had been taken towards the formation of a central authority, and the means of procuring the aid of local committees throughout the country, by addressing the governments of the several States. He mentioned the area allotted to the United States, to be 80,000 square feet.

He then gave a concise statement of the great division which had been made of the objects to be exhibited, viz:—

1. Raw materials and produce.
2. Machinery, with engineering implements and mechanical inventions.
3. Manufactures, or results produced on natural products.
4. Sculpture, models, and the plastic arts.

In the first of these great subdivisions, science will find a place in all the three great kingdoms, mineral, vegetable, and animal. The men of science of our country will find their services put in requisition to point out, and discriminate, the articles most worthy to occupy the space allotted to the products and industry of the United States.

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4. On the importance of the Theory of Bryant, Faber, Davies, and Harcourt, respecting the Deluge, to the proper Investigation of the Ethnographic Distribution of the Human Race. By J. P. Lesley, Milton.

Jacob Bryant led the way to a rational and definite study of primeval antiquity, through the manners and customs, religions and traditions, monuments and linguistic radicals, of different races and nations. Others followed him with monograms of value, such as Faber on the Cabiri, Davies on Celtic Druidism; or with ponderous and unreadable masses of unclassified and almost useless contributions, such as Harcourt's late work on the Deluge. The subject is nevertheless but just opened, and almost everything has yet to be done.

There lies back of scholastic history an unexplored world of primeval or arkite history, just as in the last century there lay behind natural history, so called, the unknown domain of palæontological research. And the fossils of that earlier human history are present for the man of science in all the soil of the actual human life. They are yet to be classified; and in their future classification lie concealed from us the laws of race and migration. They occur in all the common and vulgar words of every language, at least of all those spoken by the white race; in the whole geographical nomenclature of the central regions of the Old World; in the family names of many countries, and more abundantly in personal names and titles; in the names bestowed by tradition upon pyramids, towers, churches, caves, boulders, cliffs of strange shape or eminent attitude, promontories, needle islands, and illegible ruins; in the vulgar and classic names of minerals, beasts, birds, reptiles, fish, and fruit; in the whole literature of ancient and modern mythology, fairy tales, and nursery rhymes; in local superstitions, and even in political formulæ.

An immense outlay of scientific skill, patience, and enthusiasm is yet to be directed and expended upon this field. By three years of interrupted research, which I have been able to give to it, I have obtained little more than a general view of its magnitude and importance, and a deep feeling of the inadequate interest which it has as yet excited, not so much among historians, who are expected to deal rather with books than with things, as among ethnologists and naturalists, into whose province it falls, and whose task is always strictly one of comparison and induction. It has been left to the prophetic but undisciplined fancy of literary enthusiasts, and has fallen under the derision of sober

minds, because it has not yet been subjected to the right processes of science. By this paper, my only object is to call attention to the theory, by a few examples of its methods and results.

The theory is, that the Noachian Deluge actually happened, according to the assertion of general tradition; that it closed an earlier history of the human race, and opened for it a new one; that it became the great event of the new history, and impressed its mark upon it permanently; that after the Flood, when worship was offered, it was always in connection with recollections of the Ark, the Mountain, the Waters, the Dove, and the Noachidæ; that the original names of these became radical terms, common to all languages; were mixed up with all grammars; employed by all priestcrafts, in all rites; were shrouded in all mysteries, transmitted by all traditions, applied to all possible symbols and to remarkable objects of nature, terrestrial and celestial, and transferred to other such in all parts of the globe, by all migrations; that they governed the origin of all customs, and the erection of all public architecture, and frequently directed the lines of commerce and the policy of international law; that their first meaning was, however, gradually lost, or retained only by priesthoods and secret societies; and that all this is to be recovered and demonstrated by the remains of antiquity.

This theory I have seen reason in my own work to modify thus:—That the Deluge, wherever it happened, probably in Western Asia, was of limited extent, and affected only the Caucasian, white, historic, central race, or a part of it; that, therefore, all its consequences are primarily proper only to that race, or a part of it; but that they have been trannferred partially, at different times and in different ways, to the Mongol, beardless, and even to the Arctic races, on the one side, and to some of the African and Australian, and even to the American races, on the other. The main line of propagation I find to be, as was to be expected, an east and west zone across the central region of the Old World, stretching from the Atlantic, over India, into the Central Pacific, and crossing, as I am almost prepared to believe, the central portion of America.

I shall confine my illustration of this theory entirely within the limits of its geographical argument, and do little more than exhibit the distribution, over this zone, of one and the chief of the radical arkite names of which I have spoken. This radical is the word BAR. I have followed Harcourt in demonstrating its meaning, by accumulated evidence (beginning with the fact that it was the name of the sacred shrine of

Osiris), to be *ark* or *ship*. Sometimes the hieroglyphic or letter Σ (s), is added, to imply *sacred, holy;* BARIS, BARIΘ, the *sacred ark*, the *holy ship*.

The whole race of man, or the whole white race, or as I prefer to think, the whole of that portion of the white race descended from Ararat, being at first natural worshippers of the BAR, or BARIS, were *βαρ.βαρ.οι*, BER·BER*s*, PER·SI*ans*, PAR·SEE*s*, and were distinguished as such by the classic image worshippers. Every land which they inhabited had for one of its names, and for its most sacred name, FAR·S, BARI, BRI·tan, *land of the ship*. And if we attend sharply to the now well developed laws of the dialectic substitution of cognate literal sounds, we can trace this radical, sometimes pure, sometimes in combination with other radicals, along the whole line of the true arkite zone, from Brittan to Brazil. It is frequently preceded or followed by one of the five hieroglyphic letters—A, K, C, or G, S and D (Δ), or T.

It is necessary also to attend to the distinction established between the two branches of the white race, the Indo-Germanic and Syro-Arabian; along the double line of which, one to the north of the Mediterranean and the other to the south of it, occur the vestiges of the radical.

Along the southern line we have BAR-BAR*ia* on the east, including the whole coast of the Indian Ocean; the nation of the BARI on the Upper Nile; the race of the BER-BER*s* nomades of the whole Sahara; with their provinces BAR·C*a* on the north, and FAR·*a*N on the south; the kingdoms of BUR *Jalof*, and BUR *Sali*, on the Atlantic; and the MAUR*i*TAN*ian* BAR·BAR*y* filling up the north-west. In fact, the continent itself is A-FRI-C*a*, the continent of the sacred *Bar·k;* and in its centre lies the immense empire of BUR·NOU, the land of the *ship of Noah*. To it belongs also naturally, the aboriginal I-BERRI*ans* of Spain, with their modern home, NA·VARR*e;* and also the aborigines, or A-BORI-*gentes* of Italy, with its modern *Terra di* BARI.

Along the northern line we have FAR·S or *Persia*, then PRU·SS*ia*, FRI-S-*ia* on the German Ocean, and PARI·S, the city of Isis and the Ark. North England had its PARI·S*i* as well as France. There are B'RI-tany and B'RI·tan, two countries of the BAR. The FRA·K or *Frankish* race has sowed their holy name broadcast on the continent; they have the *dutchies of* BAR, a *kingdom of* BERRI, and a realm of BUR-*gundy*, whose ancient people wrote their name *Barbaré*. BER-N*e* was not named from its bears, nor BOIAR*ia*, *Bavaria*, from its nobles, but because it was the habitat of the ancient nation of the BURII in

Southern Germany. The Baltic was once a *mare bar-bar-um*, and on its shores dwelt the PRE·ZZ*i*. The Ural Mountains separate the great regions of PER·M*ia* and SI·BER*ia*, (*Siber* meaning in the Slavic tongues *Arctic*, as Hyper·bor·ean did in the Greek.)

Returning to Asia, we have another I·BER*ia* on the western, and another BAR C*a* on the eastern side of the Caspian, with the realm of PAR·Θ*ia*.

Pursuing the same course south-eastward from the adopted centre of dispersion, in the neighborhood of which we now are, we meet first with the most ancient Sanscrit race of the WAR·WAR*a*; then with the great regions of BER·AR and BA°AR in central India; then with the only pure Arkite kingdom left on earth, BUR·M*ah*; then, with the aboriginal Polynesian race of the HOR*a*FOR*a*; and with the great Island of BOR·NEO, the "Noah's ark" of the Pacific.

Where MAN·C*o* CAP·AC, "the holy man of the holy mount," landed on the new world, we have upon one side of it, PERU, PAR*ia*, and PARI*acaca*, and on the other the mysterious name BRA·SIL. Between them lies the region of the PAR*a·guay* and PAR*a·nay*.

In all these regions, peopled by diverse specific races, although more or less of one generic type, are to be shown ten thousands of instances where this aborginal and sacred term has been affixed, with or without epithets or adjuncts, to innumerable cities, towns, towers, castles, temples, rocks, caves, fountains, families, and individual men, gods, priests, heroes, children; offering irresistible evidence to its antiquity and importance.

England has its five-and-thirty BAR·*tons* and BAR-*towns*, and scores of villages, parishes, and hills ending in ·*barre*, ·*bury*, ·*b'y*, in every county. Thousands on the continent end in *bur·g*; in Sweden in ·*bro*, ·*b'y*; in Hungary in ·*Var*; in India in ·*poor*, and in Russia begin with *war·* and *per*; while FERRö islands occur in almost every sea explored by this maritime or central family of man.

The conclusion to which I have been led by an extensive series of observations, of which the above is an example, will be at once evident; and if true, is of the utmost importance to the ethnological questions of the day. It is, that national denominations must be given up as tests or guides in our future researches into the historic relationship of nations; because they are not stirpal, but *mythic*; they do not determine stock and kinship, but priestcraft and religion, and cannot point us out, therefore, the direction of actual migration, except in the very general, but only the direction of an arkite propagandism. The names

heretofore in most common use as proper to families, stocks, or races, such as Persian, Pelasgic, Celtic, German, Scythian, are actually convertible terms and religious synonymes, and distinguish, not their origin or kinship, but their sectarian mythology and worshipping habits. Sometimes, therefore, different names are discovered for branches of the same race, and sometimes the same name was borne by distant nations of probably different stock.

Ethnologists have always been puzzled to know why some Russo-*Finnish* tribes were called *Scythians*, and to account for the two *I·berias*, that of Spain, and that of the Caucasus. There are *Ger·mans* of Central Europe, and *Car·mans* of Central Asia. There were *Cul·-dees* in Scotland, and *Chal·dees* in Assyria. The Western name of *Cel·t* is found in Asia Minor. The *S·cots* were by stock *Celts*, but by name *S·cyth·*ians. There are the widest intervals sometimes to be thus bridged. The PAD*œi* of India, who eat their parents out of affection and respect, are represented by the BATT*as* of Java, the BATTa-COTas of Africa, and by the ⊥∩d*is* (?) of Brazil.

These, and many other names, are resolvable by a few arkite radicals and establish the religious, but by no means the natural relationship of the nations to which they belong; and in fact leave pretty much on one side the solution of the question whether there was one centre of dispersion for our race, or many centres, or a general creation over the whole ground occupied by each species of man.

From Ireland and Morocco to Borneo, the whole central zone of the Old World may be said to be in possession of the *Persian* or BAR·BAR race, worshipping the ark. The whole north of Asia is inhabited by a TAR·TAR race, worshippers of the TOR or Mountain; their plains are covered with miniature tumuli; and their idols hold up the same in their right palms. With the hieroglyphic ⊼, it is the ancient ΘRA·X or Thracian name, and the modern TUR·K; as *Bar* becomes both *Fra·nk* and *Phry·Gian.*

The Indo-Germanic TOR becomes, in Syro-Arabic, TEL; as the first god, BAR, of the Assyrians became the second god BEL. The I·TAL*i* were, therefore, the aborigines of the south of Europe; and the TOL·*tecs* were the aboriginal *tartars* of Mexico, to whom tradition justly assigns the pyramid of *Chol·ula*, and all the *Teo·callis.*

The worship not of the BAR, nor of the TOR, so much as of the KAR or KAL, the Cromlech or rock cell, gave name to the people of Central and Western Europe; they were all *Ker·ites, Kel·ites, Chal·dees, Cul·dees,* KEL·TS. Their natural hills were *Tors,* but their artificial

barrows were Cair·ns, and their churches KIR·KS. Their god was Mannus; they worshipped the CÆR of MON; they were GER·MAN*s*. But the term was not confined to the north-west. The *Celts* and CAR·*ians* lived side by side in Asia Minor, and the maps of the south-east are full of such names as *Car*inthia, *Car*niola, *Cor*sica, *Cor·cyra*, *Car*thage, *Cor*inth, *Ce*rasus, *Cor*one, *Cyr*ene, *Cora*, *Cor*duba, the mystic *Cer*·ne, the *Cor*·ycian cave, the *Car·paths*, or the mountains of the CÆR *of* BOODh. And over Central Africa wandered once the innumerable Barbaric GARaMAN*tes*, the Arkite Germans of the Equator, as the hordes of the savage GALL*a* do to-day, while the the MON·-GOL of Central Asia is but a dialectic invasion of the GER·MAN of Central Europe.

It appears that this radical obtained a third dialectic form, KAD, (a change remarkably prominent in a comparison of some Mexican dialects.) *Cats* and *Ceti* are instances of its adoption into mythic natural history. The very curious talismanic construction of this form which I have discovered, I will not not here speak of, further than as an evidence of its probable comparative low antiquity. It became the holy name of the Cromlech, or stone ark. It gave name to its divinity, and his Druid representatives; they were KÆD·MONS, *Cadmuses*. It gave name also to its worshippers, the KED*ites*, CATT*i*, χαττοι, GETE*s*, GOT*h·s*, and with the sacred hieroglyphic, S·COT*s*, and the whole race of S·KYΘ, the Scythians; a race hitherto as unaccountably indefinite and ubiquitous as its synonyme the KEL·T.

The fact is, that the region of each of these mythic radicals is almost conterminous and co-extensive with that of all the rest. As the *Mam*·-TOR of [TOR·BARI or] *Der·by*shire looks down upon *Chel·mer*·ton and *Cal·ver*, on the Atlantic side of the Old World, so do TAR·TARy, CAT*h·ay*, and BUR·MA, lie side by side along the Pacific coast.

I have, moreover, found them as accurately applicable to the remarkable members of the Flora and Fauna of the Indian Archipelago, as to the metals and sea fish of Europe. But I confine myself to ethnology.

The radical ARK, and its Syrian synonyme, ARN, has a very extensive range, but chiefly among the names of places and remarkable objects. The anti-arkite Greeks, however, called all the northern nations Hyper·*bor*ean bar·bar;oi, ARK·tic *bears*.

BAR passes through BEL into FIL in the south of Asia. FIL is the Indian for Elephant, and E·BUR is I·VOR*y*. We have thus the mytho-ethnological name of the *Bheels* of India, *Philistines* of Syria,

Palli and *Fellahs* of Egypt, and *FOULahs* of Western Africa. Like some others, it has a double reference to the ark, and to the mountain also. The following English words will illustrate my meaning; the same might be done from other languages. Compare *pole* and *pail; back* and *buck*et; *top* and *tub; cap* (summit) and cup. The radical TAB, explained by the Hebrew TBE, (Thebah,) the Ark of Noah, gave names to the four cities of the East, the Mysian, the Thessalian, the Bœotian, and the Egyptian THEBES; to the holy land of T*h*IB·*e*T; and to the wide-spread TAB·U system of the Pacific.

I shall instance but one more, too important to ethnological research to omit, even in this sketch, (for it is nothing more,) of the arkite theory. It is the term for *aborigines* in arkite nomenclature, AL*e*P*h*, the Hebrew name of the letter A. It occurs widely; in the ALP and OL*y*MP*ic* mountains; in all the rivers of Scandinavia called ELV·*en*, and the ELB*e* of Germany, and ALF*aios* most ancient and sacred river of Greece; in the ELVes and ORKs, [i. e., arkite aborigines,] who met the invading Goths and Germans. In fact, all the subjected, enslaved, or almost exterminated aborigines of the West, the remnants of the extreme antiquity the memory of which lingered about the mountain fastnesses where they had found their last refuge, were denominated ELV*es*, or with the sacred symbolic prefix, K·ELP*ies*, worshippers of the flood, water sprites; and of the ark, BRO·*wnies*, FAIR*ies*, PER*is*, the "old people" of fireside story, the Alphas of humanity, the primeval worshippers of the hill, the cave, and the tomb.

To avoid encumbering and confusing my subject, I have omitted mention of the numerous *local* changes and even inversions which these extremely ancient and significant terms have suffered in different lands, and need not say a word to prepare those who choose to enter upon this line of research for the great difficulties which these aberrations from their normal types present. It is to overcome these very difficulties, insurmountable as they are to mere lexicographers and comparative philologists, that the widest synthesis and closest analysis are alike and at once required; and it is in vain to seek for such aid beyond the circle of honest and experienced naturalists.

The thing to be demonstrated is, the distinction between the *marks* which nature makes to separate the human races, stocks, or families, and the *titles* which the intellectual and devotional life of these races have affixed to themselves or to one another. Hitherto, those *marks* and these *titles* have been thought to coincide; at least with sufficient

nicety for all practical purposes in the science of the natural history of man. But, in fact, they oppose, confuse, and obliterate each other; and we must begin very much *de novo* in our ethnology. Comparative philology, which has invented so many false migrations, now throws reasonable doubt upon all migrations, previous to the noon of history. Names will teach us the wanderings of Bards and Druids, but not of nations. The Gauls of Asia Minor may have been children of Noah, and the Celts of Gaul, antediluvians, converted to the faith of Noah. We must look upon the mere *names* of tribes as false signals hung out to shipwreck our explorations into their natural history.

This paper was received, but on account of the absence of the author, and the lateness of the hour, it was not read.

Adjourned, and the members soon after proceeded to the residence of William Hillhouse, Esq., in compliance with his invitation.

FIFTH DAY, FRIDAY, AUGUST 23, 1850.

SECTION OF GEOLOGY AND NATURAL HISTORY.

The Section of Geology and Natural History met at 10 A. M., in the Geological Lecture Room of Yale College.

Prof. B. Silliman was called to the chair. The minutes of the last meeting were read by the Secretary, after which the following communications were received:—

1. On the Position and Character of the Reptilian Foot-prints in the Carboniferous Red Shale Formation of Eastern Pennsylvania. By Prof. H. D. Rogers.

This communication states, that the ancient foot-prints first discovered by Mr. Isaac Lea, of Philadelphia, in the red shale formation at Mount Carbon, in Pennsylvania, and assigned by that gentleman to the red rocks of the Devonan period, belonged really to the carboniferous red shale, and are, therefore, of an age essentially later than that attributed to them. They occur, indeed, in a geological horizon, only a few hundred feet below the conglomerate which marks the beginning of the productive coal series, in which series similar foot-prints, attributed to batrachian reptiles, have been previously met with in

Western Pennsylvania. Instead, therefore, of constituting a record of antique reptilian life, earlier than any hitherto discovered by at least a whole chapter in the geological book, they carry back its age only by a single leaf. The surfaces upon which these interesting foot-prints abound, are the smooth divisional plains separating the beds of red sandstone, and are invariably coated with a fine impalpable material of a once slimy and soft mud; and everything in the texture of these surfaces goes to prove that they were in contact with the air, and were the stages of rest between the alternate depositions of the strata. Many of them are covered with ripple lines and water marks, suggestive of a shelving shore, and, with few exceptions, they are spotted over with little circular impressions, imputed to the pattering of rain. All over the successive floors of the living world, as delicate and impressible in their texture as so much wax or parchment, are the footsteps and the trails of various creeping things—the prints of some unknown four-footed creature, thought to be reptilian in its nature, but of whose true affinities the author expressed his doubts—trails analogous to those of worms and molluscs, and various other marks, written in hieroglyphics too obscure to be interpreted. The larger foot-prints are, for the most part, five-toed, alternate in the steps, and with the fore feet nearly as large as the hind ones. Marks of the scratching and slipping of the feet, and the half effacing passage of the tail, or of some soft portion of the body, are often distinctly legible.

Prof. L. Agassiz followed with remarks on the character of these footprints, and showed how they might have been made by the pectoral and ventral fins of fishes of an ancient type, which probably had some power of locomotion out of water.

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2. On Tertiary Fossils of Marshfield, Mass. By Dr. C. T. Jackson.

[*Not received.*]

Prof. Agassiz stated, that he had a collection of similar fossils from this locality confirmatory of Dr. Jackson's views of the identity of the Marshfield bed with the cliffs of Gay Head in Martha's Vineyard. Prof.

A. announced also the discovery, by Prof. Frazer, of the head of a walrus on the shore of New Jersey, indicating the prolongation of the Gay Head deposits.

Prof. HENRY D. ROGERS remarked on the submarine situation of these miocene beds of New Jersey, and also on their existence in New Jersey resting on the green sand.

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3. THE GENUS AMIA, A TRUE LIVING REPRESENTATIVE OF THE OLD FAMILY COELACANTHI. By Prof. L. AGASSIZ.

[*Not received.*]

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4. ON THE PROBABLE AGE OF THE DEPOSITS CONTAINING THE REMAINS OF EXTINCT COLOSSAL BIRDS, IN NEW ZEALAND. By REGINALD NEVILLE MANTELL, C. E., of London, England.

The interest excited at the meeting of this Association last year, by Professor Chase's account of the bones of the Moa, or Dinornis, from New Zealand, and the interesting descriptions which followed relating to the existence of those colossal birds within the historical period, induce me to submit to the present Assembly a few remarks on the question as to the contemporaneity of the last of the race with the Maoris, or aboriginal human race, of those countries.

On my return to England from the United States, in the spring of the present year, I found that my father (Dr. Mantell, of London,) had received, in November of 1849, another and highly interesting collection of bones of Dinornis, and several other genera of birds, some of extinct and others of living species, obtained from the middle island of New Zealand, during a survey of that part of the country, in the capacity of Government Commissioner for the final settlement of the native territorial claims. This series of fossil bones consisted of several hundred specimens, referable to the extinct genera, designated by Professor Owen, Dinornis, Palapteryx, Aptornis, and Notornis, and to the recent Apteryx, Nestor, Southern Penguin, and Albatross, and several undetermined species. With these bones were associated those of one species of Dog and two species of Seal. These remains were chiefly obtained from a turbary deposit, composed of vegetable matter, sand,

and loam, forming a very limited bed on the shores of a little creek near the mouth of the river Waikouaiti, at Island Point, on the eastern coast of the Middle Island, and about seventeen miles north of the Scotch settlement at Otago. This ancient swamp, or morass, is covered by the sea, except at the lowest tides, and is rapidly diminishing from the action of the waves. Among the specimens obtained by my brother from this spot, were the entire series of bones composing the pair of feet of the same individual Moa, the one being about a yard in advance of the other. The lithograph I have the honor of placing before the meeting, represents one of these feet, reduced to one-third linear the size of the originals. The upper extremities of the two shank-bones, or tarso-metatarsals, were alone visible on the surface of the soil after the recession of the tide; and it was by carefully digging down and extricating bone by bone, and numbering each as they were exhumed, that the matchless specimens now in my father's possession were obtained. These are the first known examples of the normal series of the bones of the foot of the Dinornis hitherto obtained, those in the Hunterian Museum in London having been arranged in their presumed original position from analogy. The circumstances under which these extremities of the Moa occurred, indicate that the bird had sunk into the morass, and, unable to extricate itself, had perished on the spot. The skeletons of the gigantic Irish Elk have been found in the ancient peat-bogs of Ireland, under similar conditions.

In a paper on the Geological Structure and Organic Remains of the Middle Island, read by my father to the Geological Society of London, and now in course of publication in the Quarterly Journal for the present month, a full consideration of all the facts bearing on the age of the ossiferous deposits, and on the probable period of the extinction of the colossal types of the Moa, is given; and to this memoir I would refer for details which it would be irrelevant for me to enter upon. I will, therefore, only state the conclusions which the data at present known seem to establish; and I may add, that in the discussion which followed the reading of the memoir, the President, Sir Charles Lyell, Prof. E. Forbes, and other eminent geologists, expressed their concurrence in the following views communicated by my father.

At a period geologically recent, but of immense antiquity in relation to the human inhabitants of New Zealand, that country was densely peopled by colossal birds, of species and genera now extinct. Ages ere the advent of the Maoris, or native tribes, the Moa and its kindred were the chief inhabitants of those islands; and from the period when man

took possession of the land, these colossal types were gradually annihilated by human agency. That some of the largest species of Dinornis were contemporary with the Maoris, there can be no reasonable doubt; for, apart from native traditions, and songs and tales, in which allusions are made to the gigantic magnitude and flowing plumage of the Moa, the collocation of calcined and half-roasted bones of the Dinornis, of dogs, of men, in the ancient fire-heaps of the aborigines (as I mentioned on a former occasion), and the unequivocal marks of the stone celt or axe on some of the leg-bones—the chipping and cuts having evidently been made on the bones when recent—afford incontrovertible proof that the last of the Moas, like the last of the Dodos, was annihilated by human agency.

From the remarkable size and strength of the limbs of the Moa, it is clear that they were powerful locomotive organs; and when we consider the vast swarms of the largest species which must have existed at some remote period, it seems highly probable that this family of colossal birds—a family unknown in any other part of the world—was no originally restricted to the narrow geographical limits of modern New Zealand, but once ranged over a vast continent now submerged, and of which Philip and Norfolk islands, and Chatham and Auckland islands, and those of New Zealand, are the culminating points.

Prof. SILLIMAN exhibited specimens of the bones.

Prof. C. B. ADAMS mentioned the existence, in the Cabinet of Amherst College, of the leg and foot of a Palapteryx from New Zealand.

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5. ON THE AGE OF THE METAMORPHIC ROCKS OF EASTERN MASSACHUSETTS. By Prof. L. AGASSIZ.

[*Not received.*]

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6. Singular Growth of a Potato. By S. Weber. Read by the Secretary.

In the autumn of 1849, a neighbor brought to me as a curiosity several potatoes, of the growth of the preceding year, and which had been kept in his cellar through the summer. The potatoes were of a middling size, white and round. The eyes appeared to have begun to germinate, and then withered, without pushing forward any shoots, as is customary, but each tuber had a rent in it of an inch or more in length, through which protruded, or might be seen, another tuber of about the size of a bullet, of apparently perfect structure, seemingly formed in the interior of the larger one, which it had burst open by its growth.

I carefully broke open two or three of these potatoes, to examine this new mode of propagation, by which the vegetable became, as it were *viviparous*. This examination showed a shoot, the 12th of an inch in diameter, and an inch or more in length, running backwards from one of the withered eyes towards, or through, the centre of the potato, and tapering at its internal extremity to a point. To the side of this shoot the new tuber was attached. The shoot was covered with a fine skin, and, though closely embraced by the substance of the parent tuber, did not seem to be connected with it, except at the extremity where the eye was situated. The substance of the old tuber was firm and juicy, though the surface was very slightly wrinkled and withered in look.

After lying in my office some days, about the first of November one was put into a flower-pot with some fresh earth from the garden, and transferred to my kitchen. The room being quite warm, the earth soon became very dry, and once or twice, when I happened to notice it, I poured on a little water. About the beginning of December the pot was removed to the cellar, where it remained unheeded through the winter. In March it was examined, and no remanis of the old tuber were found but the skin, loosely enveloping the young one, now grown to about two-thirds of the original size of its parent, and looking very plump and perfect. This new tuber was awhile ago transferred to my garden, and has sent up a very thrifty and vigorous stalk, and looks as likely to do well in the world, as if it had come into it in the natural manner.

When the flower-pot was examined in the spring, the earth was extremely dry, and nothing more than a light dust adhered to the outside of the potato.

In taking the potatoes out of the earth in which they grew, I have a

number of times noticed one enveloped, similarly to the above, in the "jacket" of an old one, and was puzzled to think how it got there; but finding a rent in the skin that thus superfluously enveloped it, concluded that from some external pressure it had passed through that opening while small, and when the old one was soft and decaying, and had there grown to its full dimensions. I now think that in such instances the young tuber was formed in the inside of the old, as in the instance above detailed, and succeeded to its parent's garment by natural inheritance.

The most interesting part, however, of the facts contained in the above statement, is in their bearing upon the question of the nutrition of plants.

It seems plain that in its early stage the young tuber, enclosed as it was in the solid substance of its parent, must have thence derived wholly its nourishment, and that it had no communication with the external air, except through the envelope of the old tuber, or the minute, abortive, and withered attempts at germination at the external end of the shoot to which it was attached, and which seemed to have perished from lack of power to support their own growth.

After the integuments of the old tuber were burst open by the expansion of the new one, the latter had through its own covering access to such amount of air and moisture as might come in contact with that, and could receive from them such benefit as might thus be imparted. While it lay in a heap with other potatoes in a dry cellar, the amount of air and moisture that could reach it was probably but small. While in contact with the fresh earth in the flower-pot, the supply of moisture was considerably increased. This period was however of but short duration, as the warmth of the room, notwithstanding one or two partial supplies of water, must have soon removed all sensible moisture from the earth, and during the last three months or more of its inhumation no fresh supply was given to it. Still it grew, and at a pretty fair rate; and the manner of its growth seemed to be in every way perfect, all due transformations seemed to have taken place, nor was there any perceptible imperfection in its organs or their constitution, so that when placed in a proper situation it rapidly and perfectly developed itself into a new plant, putting forth with vigor the very organs which were blighted and abortive in its own immediate parent.

From the coincident wasting away and disappearance of the old tuber, it seems a natural inference, that the substance of it was transferred to the new one; but then the question arises, how was it transferred? Through what organs and in what form did the starch, the matter of which the cellular texture is composed, and the other proximate ele-

ments of the potato make this passage? In part this may easily be answered. The same organization, by which the nourishment is conveyed in the common process of germination to the radicle and the plumule of the new growth, undoubtedly went into operation. From some not very obvious cause, the growth of the plumule was blighted, while that of the radicle was not. But here begins the difficulty. Though the radicle may and does in ordinary cases grow awhile on the nourishment derived from the parent tuber, yet it does not begin the formation of new tubers till the plumule has obtained its organization, and some considerable size as a plant, having its leaves expanded to the influences of light, air, and moisture, and while the radicle itself is well supplied also with warmth and moisture, and by means of its situation in the earth enabled to imbibe from it the materials needed for growth; these materials being, according to the commonly received doctrines of vegetable growth, elaborated and made perfect by passing in the circulation of the plant through the stem and leaves, and being returned to the root. In the present case the radicle seems to have penetrated backwards into the parent tuber, where it lay in contact with juices and principles already elaborated. The plumule ceased to grow, and thence was an entire absence of what are usually considered the organs by which the water, carbonic acid, ammonia, &c., imbibed from the earth, are changed into the proximate constituent principles of vegetables. Unless, therefore, the new root, when by its tuberous growth it came into contact with the earth, had the power of imbibing and elaborating for itself the ultimate principles above mentioned, it would not seem that any nourishment could be imparted to it by its being placed in the earth, except a small supply of water. The other materials of the growth of the new tuber must have been derived from the parent; but then again the same question occurs. In what state did the radicle take them from the old tuber: as ultimate or proximate principles? In the common germination, while, as above stated, the radicle derives its nourishment from the parent tuber, a deposit of diastase round the eye acts upon the starch of the potato, converting it into grape sugar, which is conveyed by appropriate vessels to the radicle and plumule for their nourishment and growth, till they become able to supply themselves by their own organs. Was the new tuber supplied in this way, and was the whole substance of the old one thus changed and converted into grape sugar, and reconverted into its original substance, by the vital action of the new one? If so, whence came the quantity of diastase necessary for this change, and by what system of vessels was the whole

substance of the old tuber conveyed to one only of its eyes? If it be supposed that the old tuber decayed, and was resolved into its ultimate elements, how were these elaborated and again formed into the proximate ones? This is the same difficulty as was met with above with regard to nourishment drawn from the earth.

It seems necessary, then, either to admit that roots have in certain cases the power of elaborating for themselves and forming into their own constituent compounds the ultimate vegetable principles, or that they have the power of appropriating to their own use such compound principles already formed.

Either of these things meet with strenuous opposition in the doctrines taught relative to vegetable physiology.

These doctrines were, however, for the most part, derived from the observation of instances proceeding in a manner to which this case of the potato may be considered an exception; but a careful investigation of the case may afford the means of correcting or confirming some of the views entertained upon the subject.

It may be, that the development of a tuber of a new growth has some laws peculiar to itself, which have not yet become familiarly known, and which may explain the above case, or to a knowledge of which the case itself may serve to lead.

Professors Agassiz and Bailey objected to the author's explanation.

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8. Essay on the Classification of Nemertes and Planariæ:—Preceded by some General Considerations on the Primary Divisions of the Animal Kingdom. By Charles Girard.

I.

I am gathering materials for a monograph of the Nemertes of this side of the Atlantic. I have already said, on another occasion, that I was doing the same with regard to the Planariæ. It will not be anticipating the final results of these investigations, to present here some general considerations respecting the place that may conveniently be assigned to these animals in our zoölogical system.

The chronological history and the position that the various authors have assigned to the Nemertes and Planariæ having been fully brought out by Mr. de Quatrefages,* I shall not dwell upon that part of the subject.

The question now under consideration bearing merely upon the fundamental groups of the animal kingdom, I need only go back to Cuvier, since he established these groups, and since the foundations of our classification are the results of his labors.

Nor shall I at present enter upon the secondary divisions to be established among the Nemertes and Planariæ; this aspect of the question cannot be settled until we are better acquainted with the organization of the group of Rhabdocœlæ or fresh-water Planariæ.

II.

Before, however, I treat of the Nemertes and of the Planariæ in a more particular manner, I have a few words to say on the subordination of characters in the primary groups of the animal kingdom.

There are in the animal kingdom but two main systems of organs, to which all the others are related, being, in a word, mere dependences or branches of them. These two systems of organs are: (1,) the *nutritive system*, or *system of vegetative life*, and, (2,) the *sensitive system*, or *system of animal life*.

Now, which of these two systems of organs is the more important; in other words, which ranks higher as a zoölogical character? The nervous system, some physiologists will answer. But let us examine this point. What is the distinguishing character of the animal kingdom as a kingdom? A digestive cavity of some kind, into which are introduced nutrient substances necessary for the maintenance of living bodies; necessary to their growth anterior to the period of full development, and to their equilibrium after having reached their complete size. The digestive system then is the universal character, common to all animals, and this character gives us the animal kingdom; it ranks then highest.

Next to the entire animal kingdom, we have the four great primary divisions, the division of Vertebrata, the division of Articulata, the division of Mollusca, and the division of Radiata; and these four divisions are characterized by the nervous system chiefly. The plan of structure of the nervous or sensitive system giving these divisions and these

* Ann. Sc. Nat., 3d Série, iv. 1845, p. 129.

divisions exclusively, its importance is of a secondary degree. For the nervous system has not yet been materially demonstrated in all the Radiata, whilst the nutritive system is to be seen everywhere. Not that I deny the sensitive system to any animal, even where it has not been shown. There exists among the lower Radiata a homogeneity of substance, which is perhaps the only obstacle to its discernment; nevertheless, the digestive system being everywhere distinct, this latter must have the pre-eminence.

It has the pre-eminence because it gives us the unity of the kingdom, as we have also this unity in the perfect resemblance of all eggs at an early period of their history. In the animal the first substance which is formed is the vitellus or yolk; it is the foundation of the future being; it is, as Prof. Agassiz has observed, the being itself. Out of the yolk the nervous system originates, as well as all other parts of the organism, so that in an embryological point of view the nervous system holds a second rank.

It is of secondary rank as a material organ and character of classification of animals. It plays the highest part by its immaterial essence in the human species. But here we leave the boundary of the animal kingdom, and therefore the classification, which is our object, to enter another kingdom, the kingdom of Thought.

The nutritive system being the index of animality, we see all animals equally compelled to take food; this is the essential condition of their existence.

The nervous system being the fundamental basis of the primary divisions, it gives to each division a special immaterial tendency, so long taught by Prof. Agassiz. Now, as there are four divisions, there are also four of these tendencies. And as soon as there are four immaterial tendencies, there is an antagonism amongst them. This is a natural consequence, since the nervous system overrules the *division*, and its dominion is of a spiritual character.

The nervous system stamps upon the *divisions* their zoölogical form as the symbol of their diverse tendency. We shall see further an *apparent* anomaly of this kind in the beings placed at the boundaries of two divisions, where the material form, to use the words of Milne Edwards, escapes the supremacy of the nervous system. The principle, however, remains always the remote cause.

Besides the antagonism of the divisions between themselves, there is an antagonism between the *instinct* and the *intellect*, that is to say, three of the divisions against the fourth; the Articulata, the Mollusca, and the

Radiata, collectively known under the appellation of invertebrate animals, all of which have only instincts, against the Vertebrata, which possess, besides instincts, intelligence to a certain degree.

Now, between the invertebrate animals and the vertebrate the struggle is latent, passive, because the two principles, the instinct and the intellectnood, cth ëxist. But among Vertebrata, where we find both the instincts and the intellect in the same individual, the struggle is active and direct, and we are indebted to the observations of Fred. Cuvier, for the revelation of this astonishing law, that wherever the instincts command, the intellect is actionless, and wherever the intellect governs, the instincts are silenced or nearly so. There is a struggle and an open struggle; the victory of one of the principles involves the subjection of the other.

Morally speaking, we might thus establish two series in the animal kingdom; an instinctive series, stationary and sightless, and an intellectual series, progressive and seeing; the first including three divisions, (Radiata, Mollusca, Articulata,) and the second only one (Vertebrata.)

And now, if we go back to the origin of the animal kingdom, and trace its history in the past, we see the two series appearing simultaneously from the first manifestation of life upon the surface of the earth: the instinctive series with its three divisions and all the classes; while the intellectual series is represented by one class only, that of fishes.

Their forms are renewed during a succession of periods, and each great revolution of the globe adds one class to the division of Vertebrata or the intellectual series, in the order of their zoölogical gradation: Reptiles, Birds, and Mammals:—then Man crowning the whole work. Thus a real progress is manifested in the division of Vertebrata, while the Invertebrate animals remain what they were, although undergoing a renewal of forms. The reason of this is, as Fred. Cuvier states, that the instinct is innate, always sightless, necessary, and unchangeable, whilst the intellect is progressive, conditional, and susceptible of modifications.*

In the intellectual series there was an aim, a design, and this was, to arrive at man, the true domain of intelligence. This aim realized, the creation would stop, and it did stop. Zoölogical forms had acquired all that diversity with which the sphere of activity of each division was endowed. To the immaterial principles nothing was left except a limited play, a contest for supremacy. To intelligence alone was given the

* Flourens, Résumé des travaux de F. Cuvier, sur l'instinct et l'intelligence des animaux. Paris, 1844.

power to arrive at the knowledge of the actual world, to look back in time, to contemplate itself in the past in view of the future, finely to study itself,—in a word, to *reflect*.*

The power of reflection belongs exclusively to man, the last being created. Man being the converging point of the material creation, in him were also to be concentrated in our time the struggles of the two spiritual principles of all past times.

One word more on the intellectual series. The fishes, reptiles, birds, and mammals belong to this series; but the fishes, the reptiles, the birds, and most of the mammals, in their natural condition of life, have no intelligence,—have no intellect.

The intellect resides within the brain, and the Vertebrata alone have a true brain. The brain is composed of several parts. There is the base of the brain, which sends nerves to the organs of sense, and the hemispheres, the special seat of the intellect. Now, of the hemispheres, the fishes have only a rudiment, and this is the reason why they have no intellect. There exists a well defined progression from the fishes to the mammals with respect to the development of the hemispheres; placed anteriorly in the fishes, they rise degree by degree in the other classes over the base which is gradually covered and concealed under them. Here we see the organ reflected upon itself, reminding us of its function in its full activity, *reflection*. To this gradual development corresponds a position of the head more and more raised, which becomes vertical in man, where it forms a right angle with that of fishes. One step more would have been retrograde: the development there stopped.

Thus, by a gradation almost imperceptible, we have beings belonging to the intellectual series which have the intellect only in a virtual state. They have the organ without having the principle; or at least admitting the principle virtually present, the organ is not sufficiently developed to allow its manifestation.

These general considerations, although a brief résumé, will perhaps appear out of place in this paper; but my object is the discussion of the value of the nervous system as a zoölogical character, and to show that while this system of organs gives only the divisions, these latter are governed by it in an absolute manner.

I now come to the special topic of my communication.

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* Flourens. Résumé des travaux de F. Cuvier, sur l'instinct et l'intelligence des animaux. Paris, 1844.

III.

The place assigned by Cuvier to the Nemertes and to the Planariæ in his "Animal Kingdom," is entirely provisional, as acknowledged by the illustrious naturalist himself.

The Nemertes are placed in the division of Radiata, immediately after the *Intestina cavitaria.*

Between Nemertes and the intestinal worms of this order there are only analogies. The extraordinary length of the body of some of them, for instance *N. Borlasii* (Borlasia angliæ), a length reminding us of the class of worms, and, above all, of some of the intestinals, such as the tape worm, had prevailed over all other considerations. Their affinities were not acknowledged, because their organization was unknown.

At that time the intestinal worms were regarded as Radiata, for the reason that their nervous system had not been found, and the Nemertes, as well as Planariæ, were regarded as intestinal worms, because all of them reminded us, by their forms, of the forms of these last.

When the more recent labors of some zoölogists had established beyond any doubt that the intestinal worms belonged to the division of Articulata, on account, first, of their having a nervous system, and a nervous system constructed on the plan of that group, secondly, by the structure of their body, which is composed of a series of articulations or rings movable upon each other, then the Nemertes were carried with the *Intestina cavitaria* into the division of Articulata, where they remained as little known as before. It is but of late that they have been made the subject of a special study by a skillful zoölogist, Mr. de Quatrefages, and I am surprised that this author has not pointed out the close affinities which they bear to Mollusca.

Cuvier was well aware of the space which separated Nemertes from intestinal worms, inasmuch as he foretold that they would one day constitute a new order. In spite of these external resemblances, their structure, which as he says "*is of an extreme softness,*" caused him to doubt. Nevertheless it did not, on this account, enter into his mind to compare them with molluscs.

At that time, as indeed now, the idea of a mollusc corresponded with the idea of a shell-bearing animal, with the form of a body more or less drawn together into itself, while the lengthening of the body involved by analogy the idea of a worm.

Now if, abstracting the form, which is not the characteristic of the

divisions, we look at the intimate structure, if we give up also the shell as circumscribing the division of Mollusca, we shall find in the Nemertes all the principal characters of molluscs: a soft body, entirely smooth, covered with a glutinous mucosity; a very simple nervous system reduced to a small number of cephalic ganglia, whence nervous threads depart to distribute themselves in the body. If we further state that, as in the greater number of molluscs, the surface of the body is covered with vibratory cilia, which help their movements, movements generally slow, deprived of energy, then we directly arrive at the idea that Nemertes are really molluscs,—molluscs of a low rank, being parallel with the worms of the division of Articulata by the analogy of their forms.

Having discussed above the value of the nervous system as the predominant character of the *division*, exclusively of any other character, it remains only for me to state that the nervous system of Nemertes is constructed upon the plan of the nervous system of molluscs: there exists a cephalic mass, more or less lobed, representing either the superior æsophageal ganglion of other molluscs, or that same ganglion to which, on account of the peculiar form of the body, are added the two or three abdominal ganglia. Nervous threads are distributed in all directions; two of them, more voluminous than the rest, but uniform in structure, run along the sides of the animal, sending off thinner threads, without showing in their course those ganglia or swellings which distinguish the nervous system of Articulata, such as it is in Malacobdella, Peripatus, &c.

The disposition of the nervous system of Nemertes, then, is merely *analogous* to that of annelids; its structure is that of the nervous system of molluscs.

IV.

The position of Planariæ in the division of Radiata is not less curious than that of Nemertes. Included in the second order of intestinal worms, the *Parenchymata*, they are brought near the Trematodes, to which they have only analogies, in the same sense as those that Nemertes have to the *Intestina cavitaria.*

The *Intestina parenchymata* have been withdrawn from the division of Radiata and brought into that of Articulata, and for the same reasons as that of the *cavitaria*. The Planariæ, of course, have thus been compelled to follow them in the same manner as Nemertes have followed

the latter. But also little investigated at that time, their affinities with molluscs have escaped the eyes of zoölogists.

The Planariæ are not parasitical, as Trematodes are, and it is important that this fact should be noted, parasitism existing most extensively among Articulata. The investigations of Mr. de Quatrefages and others have moreover made us acquainted with their structure, which, although not yet entirely understood, as I believe, is however of the highest interest. Indeed, in Distoma the digestive system is constructed upon an analogous plan with that of Planaria; but the digestive system, as we have said, cannot characterize the primary *division*. It characterizes the kingdom.

As soon as it is acknowledged that the *division* rests upon the structure of the nervous system, the fact that the nervous system of Distoma is that of Articulata, and the nervous system of Planariæ is that of Mollusca, there is no ground for further hesitation.

In Distoma there are two cephalic ganglia, whence nervous threads depart for the anterior region of the body. From each one of these ganglia arises a nervous thread, which takes its course backwards; this thread presents on its length a series of small ganglia, scarcely distinguishable upon the middle region, it is true, but very apparent towards the posterior region, where they are seen sending off smaller threads, distributing them to the body. This arrangement of the nervous system of Distoma becomes especially distinct in Malacobdella, where the same arrangement is found, with the ganglia of the lateral threads more developed. Thus, taking the nervous system into consideration, Malacobdella is one degree higher than Distoma; then Clepsine would follow, in which the two threads are brought so close together that they combine into one single thread. Above Clepsine would rank the other Hirudines.

In Planariæ we have a cephalic ganglion, more or less lobed on its circumference, which sends nervous threads to all the regions. There are two more voluminous lateral ones (one on each side of the body), as in Distoma, but uniform, as in Nemertes, still recalling here by analogy the nervous system of Articulata. The fundamental difference, although less apparent at first sight, consists in the absence of ganglia upon their lengths, and this fact decides all.

The body of Distoma, indeed, is not articulated, and this may perhaps lead to a belief of a closer affinity between Planariæ and Worms. Their broad and flattened form was better adapted to their mode of life without that structure. Moreover, the articulation of

the body, although a character of the division, is subordinate to the nervous system. Consequently, there is no ground for surprise at seeing it vanishing or even entirely disappearing in those groups placed at the confines of two divisions as a material litigious property disputed by antipathic vital tendencies, each endeavoring to appropriate it to itself.

Considering now the softness of Planariæ, that glutinous mucosity which surrounds them, that body of a uniform shape without articulations and deprived of articulated limbs, that general apathy, all these are so many characters which they partake with Mollusca, and do not partake with Articulata.

However, I do not know that any one has proposed to consider these animals as Mollusca, although Baer and Dugès had already compared the inferior disk of Planariæ to the foot of Gasteropoda, without thinking of bringing them together in the same natural group. Mr. de Quatrefages refutes this comparison. We shall come to it again presently.

For, if the shell does not characterize Mollusca, inasmuch as all living Cephalopoda are naked, and among Gasteropoda, we have the whole group of Nudibranchiata deprived of a shell, that of Pteropoda, and among Acephala that of Tunicata, it requires no effort of imagination to admit in that division animals such as Planariæ. They are flattened molluscs, in the same manner as Nemertes are elongated or stretched molluscs, as are also Dentalium, which nobody now would place elsewhere than among Mollusca.

Many authors have spoken of the organization of Planariæ, from Dugès to MM. de Quatrefages and Blanchard. All have viewed them as worms, doubtless prepossessed by the idea that Cuvier, who had established the divisions, could not have been mistaken so far as to place side by side in the same order animals belonging to two different divisions.

But this error of the author of the "Animal Kingdom" is easily accounted for. At that time he was in want of the essential datum to settle such a question in its details, the knowledge of the nervous system.

The plan of structure of the nervous system, we have already said, and we cannot repeat it too often, gives only the *division* and nothing but the division. Now, Cuvier did not know it when he laid the foundation of his classification; although he foresaw the four plans of organization of the whole kingdom; and, as he said, there are but four of these plans.

In reading the history of the Earth on the strata which compose its crust, as so many pages of a book written by the hand of the Creator, we then find again the thought of these four plans of organization.

These four plans of organization would acquire a far greater importance if embryology should ratify them. Now embryology does so. All embryological investigations, past and contemporaneous, lead towards four plans of structure. I shall not treat of this question more in detail here; it is sufficient merely to mention the fact.

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Now the question respecting the class among Mollusca to which Planariæ belong, is easily settled. They crawl on the inferior surface of their body; that they are gastropods, there can be no doubt. Do we not see them, as well as Pulmonata, Nudibranchiata, and others, creeping along the walls of a basin, and, when near the surface of the water, reversing their position and walking in that manner with the same facility? Do we not witness the same undulatory contractions in that foot? Therefore, were not Baer and Dugès guided aright when they compared the inferior surface of the body of Planariæ to the foot of gastropods?

I am not aware that any of the Planariæ I have studied moves with the same facility on the back as on the belly, as Mr. De Quatrefages states. Whenever I placed them in that position, I always saw them changing it as quickly as possible. Besides, in most cases I have seen the upper surface differing widely from the inferior one.

In Planocera we have on the dorsal surface two cylindric tentacles, analogous to the cephalic tentacles of doridians. In Thyzanozoon or Eolidicera, the cephalic tentacles are flattened, and are brought entirely forward, while other tentacles appear on the back, reminding us of the dorsal appendages of eolidians proper. Then come Proceros and Procerodes, with but the flattened anterior tentacles of Eolidicera, and in the same place as in these last.

There exist, then, in the marine Planariæ one group which reminds us of Doris, another of Eolis, and still another slug-like group, the *Procerodians*, intermediate between eolidians and the fresh-water *Planarians* proper.

This shows that it is near Nudibranchiata that Planariæ will find a natural place, and on several accounts we should be tempted to consider them as a degradation of that type.

But for us, who do not admit the so-called *degradations*, inasmuch as each being appears to us, as to Cuvier, *a perfect whole by itself*, we consider Planariæ as a family nearly allied to that of Nudibranchiata, bearing in itself the reason of its existence as strongly defined as this latter, and representing merely a variation in the Thought of the Creator.

For, in Nudibranchiata we find a number of types all as much diversified. These are: *Doridians*, *Eolidians*, the slug-like *Canthopsis* analogous to Procerodians, and which leads to the *Acteonians*, then finally the genera *Pelta* and *Chalidis*, which constitute another group almost planarian, deprived of external appendages, analogous to the terrestrial species of Planaria.

There exists a striking parallelism between the zoölogical forms of these two families.

Viewed in the light of their organization, nothing is more alike. The nervous and digestive systems scarcely differ. I have already spoken of the first. With regard to the second, I shall recall the fact that it is ramified in Nudibranchiata as in Planariæ; the ramifications being diversified according to the groups.

No respiratory organs, properly so called, in either Nudibranchiata or Planariæ.

The organs of generation do not differ much more. Nudibranchiata are androgynous like Planariæ. The fecundation takes place by mutual impregnation, as is the case in Pulmonata. But in Planariæ we have cases where an individual fecundates itself, the hermaphroditism being here complete.

V.

It still remains for me to make some general remarks upon Nemertes and Planariæ.

Mr. De Quatrefages tells us himself: "Neither Nemertes nor Planariæ have externally a resistant and tough layer, similar to that which is found with annelids, for example, or even with Rotatoria."*

We have, then, two groups of gastropodous molluscs parallel with two groups of annulated Articulata; the group of Planariæ reminding us of Helminthes, and the group of Nemertes reminding us of the Hirudines.

At the extremes of these two groups, at the bottom of the two classes, we witness in some sort a strange and opposed struggle of the two immaterial principles of the divisions which exert themselves to

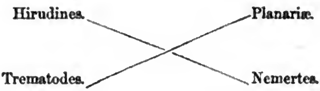
* Am. Sc. Nat., 3d series, vol. iv. 1845.

take from each other some portion of their material property. As examples, we have:

In Mollusca,—the Nemertes, which elongate and become worm-like; the Planariæ, which remain shorter but pressed down, spread out, flattened in thin leaves.

In Articulata,—The softness of Helminthes in general, the flattening of their body in Trematodes, in which the articulation of the body vanishes, analogous to Planariæ;—the softness still of the Leeches with a distinct articulated structure, being parallel with Nemertes.

These groups do not oppose each other in an exact parallellism; for Nemertes, which form a low type of gastropods, are opposed to the Leeches of a higher grade of worms, and Planariæ, a higher grade among gastropods, are opposed to Trematodes, a lower type among worms. In this manner:



In the elongation of the body of Nemertes there is nothing to surprise us. Placed at the bottom of the class to which they belong, they assume a form analogous to a group of the division of Articulata which attract them, but to which they do not belong. Now, when a mollusc, whose body is elongated beyond all proportion, and obliged sometimes to move by the contraction of transverse muscular fibres, accelerates its progress, then we see that mollusc assuming certain transverse, irregular, and unequal folds—shadows of articulations which in reality do not exist.

At the bottom of the division of Articulata we observe similar facts. The Trematodes lose insensibly that elongated form of the body which constitutes the prominent character of the worms; they are flattened, spread out, meanwhile the articulation of the body, the characteristic of their type, vanishes completely,—thus foreshadowing the type of Planariæ.

The position and number of eye-specks in Nemertes and Planariæ indicate also a greater resemblance to the same organs in molluscs than to those of annelids. When eyes exist in annelids, they are arranged in pairs on both sides of each articulation, or else form a crown on one of the anterior rings. In Planariæ we find the eye-specks irregularly grouped on the upper surface near the anterior region of the body.

The same arrangement is observed in Nemertes. This arrangement forcibly remind us of what we see in gastropods. In Planariæ alone they are more numerous, and distributed with less constancy, a fact which is accounted for by the lower position of that family in the class.

The habits of Nemertes and Planariæ speak more strongly in favor of molluscs than worms. Most of the species live concealed under stones, which is the case with many molluscs, while I do not know any which lives within a tube constructed like those of worms, even in the most elongated Nemertes.

Moreover, in the southern hemisphere there exists a group of terrestrial Planariæ, whose habits and characters are, if possible, still more molluscoid. They were observed and described by Charles Darwin, who thus expresses himself on the subject. "They may, however, form a section of the genus [Planaria], being characterized by a more convex and narrow body; their more distinctly defined foot; their terrestrial habits; and frequently by their longitudinal bands of bright color. From their colors, from their convex bodies, from their manner of crawling and the track of slime which they leave behind, and from their places of habitation, they present a striking analogy with some terrestrial gastropods, especially with *Vaginulus*, with which snails I have several times found them associated under stones."* This shows not only a *striking analogy*, but the greatest affinity with Molluscs. Those air-breathing Planariæ, with their convex bodies, immediately recall to our mind the terrestrial slugs; and were it not that they want the cephalic tentacles of the latter, and possess a ramified intestine, they would at once have been placed among Molluscs. If land-slugs have a simple intestine, in the sea-slugs it is ramified, showing that on this ground alone they could not stay among worms.

The embryonic development of both Nemertes and Planariæ takes place according to the laws we witness among gastropods. Even in the earliest history of the eggs, we observe similar phenomena in Nemertes and many gastropods. These pouches, in which several eggs or vitellus are enclosed, do we not observe them in both groups? The motion of the embryo within the egg-envelope, is it not seen in Nemertes and Planariæ, as well as in Nudibranchiata, and other Gastropods? The cilia which surround the embryo exist similarly in all of them. And the fact that gastropods and Planariæ pass through a larval state before they reach their full growth and resemblance to their parents, is another circumstance which ought not to be overlooked.

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* Annals and Mag. of Nat. Hist. xiv., 1844, p. 242.

It is plain enough, Nemertes and Planariæ are only *analogous* to Articulata; by their *affinities* they are Mollusca. The division of Articulata hence appears to us as a type more natural and rational, as well as that of Mollusca; this latter including all animals which are soft, slimy, and flabby, whetever may be their form.

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The Rhadobcœlæ, or fresh-water Planariæ, will be the connecting link by which the Nemertes approach Planariæ proper, as members of the same group. We may say of them, that they are the fresh-water representatives of both Nemertes and Planariæ. And now observe the transition: this group, Rhabdocœlæ, which is distinguished at once from Planariæ and Nemertes, this group, I repeat, lives in fresh waters, while the other two groups, which they connect together, are scarcely found beyond the boundaries of the seas. If some of them ascend the rivers, it is within the limits where the water still retains a part of its marine character. So that the connection, although materially expressed, exists essentially in their immaterial essence.

VI.

I now conclude by a few words on Annelids and Gastropods.

Having withdrawn from the first of these classes a certain number of its representatives to include them among the second, it is to be expected that I should present in a synoptical manner the systematic modifications resulting from it.

If we admit the two sections of worms proposed by Milne Edwards, the Pleuronera and Annelids proper, we should place them one above the other, to form a single series instead of one.

Opposite we should have the series of gastropods, beginning with Nemertes; above these the Planariæ; then the Nudibranchiata.

Worms.	Annelids.	Annelids proper.	Gastropods.	Nudibranchiata.	
		Scoleidea.			
		Hirudinea.		Planariæ.	————
					————
					————
					————
					————
	Pleuronera.	Peripatus.		Nemertes.	————
		Malacobdella.			————
		Polycladus.			————
		Helminthes.			————

What would come next to the Nudibranchiata I am not prepared to

say. The series cannot go on, on that footing, for the very simple reason that embryology assigns a lower rank to all gastropods provided with a shell, inasmuch as Nudibranchiata, when hatched, have a shell, which they lose at a later but still an early period of their life. The attempt at forming organic series in the animal kingdom is one of the most difficult of labors, and a work of a relative value. Unless several series are established in the class of Gasteropoda, the position of Nudibranchiata in the above synopsis cannot be accounted for. I have already made the remark that Planariæ were rather parallel to Nudibranchiata than of a lower rank, finding in these two families groups of equal importance.

Respecting the serial arrangement of worms, as given above, and resting upon the morphology of their nervous system, I have also to remark, that it does not coincide with another series alluded to in a remarkable paper of late publication ;* and, indeed, if lumbricine annelids and Leeches are higher in their class, as I should be willing to believe, this would corroborate the idea, that the nervous system characterizes only the primary divisions, and cannot express exclusively the organic gradation of the natural secondary groups, unless in an artificial method. Thus our position taken at the beginning would turn out to be the right one.

Now if, instead of a single series, or several series, with a starting point and a terminal end, we construct a circle or an ellipse, and place the gastropods on its circumference, then the respective arrangement of each group would meet with less difficulty, and, perhaps, satisfy completely our mind ; for viewed in that light of independent and circular groups, the Creating Thought presents itself to us harmonizing with our notions on the whole universe.

Our constant aim in the study of natural history consists in a thirst for Truth, in an aspiration for pure knowledge.

The natural Truth is defined : *the relations which exist between the Creator and the created beings ;* and we may say more correctly, *the immaterial relations*, &c.

Now, the creation being the manifestation of Thoughts, under an earthy coating, when we dare to ascend to the primitive Thought that has called into existence the whole universe, and contemplate the

* The natural relations between animals and the elements in which they live, by Prof. L. Agassiz. *Silliman's Journal*, 2d ser., vol. ix. p. 369.

Creator's mind in the work of his creation, we feel ourselves in the presence of a majesty almighty in power. No beginning, no end to be perceived in Him, as theology teaches us. We witness a material beginning and a similar end to all living beings, such is their destiny on earth. This destiny was strictly defined, their plans of structure clearly conceived, before they were called into any sort of manifestation, into material existence.

Thus two events strike us most, the so called Birth and Death, circumscribing an area within which we observe phenomena taking place. This area constitutes the circle of activity with which living beings are endowed to cross the present world. But where was the being before, and where it is to be afterwards, and how ?* There is a circle of activity for each individual, and for each sex when separated ; there are as many circles of activity as natural groups to which the individuals, the species, the genus, the family, &c., belong, according to their rank.

Without identifying the individual phenomena of the physical world with those observed in organized beings, we may find out in the various orbits and different systems of the sidereal hierarchy an equivalent of the same fundamental idea or law.

The natural groups, therefore, exist in nature, and are not mere fancies of our imagination, except in the case of artificial systems of classification.

The circle or the sphere, moreover, is the emblem of the intellect reverted upon itself ; the emblem of the reflexion in its self-possession, in its self-knowledge, in its free exercise, in its wisdom. The emblem also of the sublime omnipresence of our Creator.

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* These questions have certainly their importance to the philosophers of our century. During the last one they were discussed by all thinking men, when of sound facts we had but few, and the subject was therefore too premature. I have much more to say with regard to them, and intend to do so on another occasion.

FIFTH DAY, FRIDAY, AUGUST 23, 1850.

SECTION OF GEOLOGY AND NATURAL HISTORY.

The Section of Geology and Natural History met at 3 P. M., in the Geological Lecture Room of Yale College.

WILLIAM C. REDFIELD, Esq., was called to the chair. The minutes of the last meeting were read by the Secretary, after which the following communications were received :—

1. ON THE EARLY USES OF THE METALS AS MONEY. By Dr. J. H. GIBBON.

[*Not received.*]

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2. ON THE COAL FORMATION OF CENTRAL NORTH CAROLINA. By Prof. WALTER R. JOHNSTON.

The coal field referred to lies partly in the county of Chatham, and partly in that of Moore, very nearly in the geographical centre of the State of North Carolina. Geologically, it appears to repose directly but unconformably upon the upturned edges of the gold-bearing rocks; but of the degree of conformity between the two formations I am unable to speak with entire confidence. On proceeding from Pittsboro, in Chatham county, towards the north-easterly portion of the coal field, we cross the edges of slates, many of which are highly indurated, until within about half a mile of the openings which expose the coal. The underlying sandstone, which reposes almost horizontally upon the metamorphic slates, has a slight southerly inclination of not more than six degrees. The coal is here about three feet in thickness, and is covered with a friable bituminous slate, in which but few distinct organic remains could be detected; but many coprolites of fishes or reptiles and many minute shells are observed. In this slate are also observed thin laminæ of carbonate of lime. The analysis of this coal proved it to be of the highly bituminous kind, yielding volatile matter 32·82; fixed carbon, 63·78; and earthy matter, 3·40 per cent. The fixed carbon consequently bears to the volatile ingredients of the coal, the ratio of 1·94 to 1; or, in round numbers, 2 to 1. The color of this

coal is brownish black. Specimens occur in which are cavities of considerable depth, in which were once contained cone-shaped fossils. The specific gravity of the coal is 1·31.

Four miles to the south-west of the first point observed, and on a tributary of the Deep River, at a place known as Haughton's Mills, the coal is again observed, and has been many years worked to a limited extent for local use. The seam appears to be four feet or more in thickness, to have a somewhat higher angle of inclination than the preceding, and to be of an entirely different composition. Its specific gravity is, however, the same, 1·31. It contains, of volatile matter, only 23·63 per cent.; of fixed carbon, 72·57; and of earthy matter, 4·80; and consequently the fixed is to the volatile combustible matter as 3·07, or, in whole numbers, as 3 to 1.

The color of the coal is jet black, lustre shining, and some of the surfaces are striated in a manner similar to that of certain semi-bituminous coals of South Wales.

In a third locality, which lies within the borders of Moore county, and about six and a half miles south-west from Horton's Mills, is found coal, widely different from both the preceding. In the first place, its specific gravity is 1·55, instead of 1·31. In the second place, its volatile matter is only 6·64 per cent., while its fixed carbon is 83·76, and its earthy matter is 9·60; consequently, the relation of the fixed to the volatile ingredients is 12·61 to 1; or we may say $12\frac{1}{2}$ to 1. This circumstance, aś well as its shining lustre, its hardness, its high specific gravity, and its undergoing no proper intumescence when heated to redness in close vessels, fixes its character as a true anthracite. It stands in the range of its class by the side of the anthracites of Lyken's Valley, and some of those west of Pine Grove, in the southern coal field of Pennsylvania.

On pursuing the range of the coal formation still farther south-westward, we find the slates, it is true; but as yet without the coals, and the slates are plumbaginized, containing, of water, 11·18; of fixed carbon, 10·35; and of earthy matter 78·59 per cent. Thus, within the space of 14 miles, are found materials differing even more widely from each other than the semi-bituminous coal of Stony Creek in Pennsylvania differs from the hardest anthracite 60 or 70 miles distant on the Lehigh, the north-eastern termination of the same coal field.

Prof. Wm. B. Rogers remarked on the difficulty of determining the age of the coal beds of North Carolina and Virginia, and of the associated rocks, and on the doubtful value of such beds.

Prof. JOHNSON added some statements of the quantity of coal in the North Carolina bed, and observed that a seam of coal, of from three to five feet in thickness, would, if not in North Carolina, at least in most other parts of the country, be estimated as of some value in an economical point of view.

Prof. L. AGASSIZ observed that the fossil fishes which belong to the coal formation of Virginia, and to our so-called New Red Sandstone, indicate an age intermediate between the European New Red and the Oolite.

The discussion was continued by Mr. REDFIELD, Prof. WM. B. ROGERS, and Prof. JOHNSON.

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3. ON THE DEVELOPMENT OF COMPOUND ORGANS FROM SINGLE CELLS. By Prof. L. AGASSIZ.

[*Not received.*]

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4. SINGULAR DEVELOPMENT OF THE LIVER, AIR BLADDER, AND KIDNEYS, IN SILURIDÆ. By Prof. L. AGASSIZ.

[*Not received.*]

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5. ON THE PHYSICAL CHARACTERS OF AMERICAN MICAS. By Prof. B. SILLIMAN, Jr. Read by Prof. JAMES D. DANA.

[*Not received.*]

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6. ON THE DEVELOPMENT OF COMPOUND EYES IN ARTICULATA. By Prof. L. AGASSIZ.

[*Not received.*]

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7. ON SOME POINTS IN THE STRUCTURE OF SCLERODERMS AND GYMNODONTS. By Prof. L. AGASSIZ.

[*Not received.*]

Prof. J. W. BAILEY inquired whether these fishes swallowed air as well as water in the process of inflation.

Prof. B. SILLIMAN stated that he had seen them exploded by children in sport.

Prof. C. B. ADAMS represented the usual form of a West Indian diodon, and described the use of the process of inflation and erection of the spines as a means of defence.

Prof. AGASSIZ remarked that this statement explained the use of a sub-cutaneous muscular layer.

F. S. HOLMES, Esq., observed that he had always found water in these inflated fishes; and Prof. AGASSIZ said that they must be able to swallow either air or water, or both.

Prof. SILLIMAN inquired whether the teeth of sharks are erected by muscular fibre.

Prof. AGASSIZ said that, contrary to the general belief, he should reply that a contractile tissue raises those teeth which are fully developed.

FIFTH DAY, FRIDAY, AUGUST 23, 1850.

SECTION OF PHYSICS AND MATHEMATICS.

Prof. OLMSTED in the chair. Prof. W. B. ROGERS and Prof. LOOMIS secretaries.

The following communications were presented :—

1. DESCRIPTION OF AN INSTRUMENT FOR EXHIBITING THE MODE OF VIBRATION IN A MOLECULE OF UNPOLARIZED LIGHT. By Prof. E. S. SNELL, Amherst.

I have interested myself somewhat in attempts to devise instruments which may serve to elucidate to the learner the nature of different kinds of waves. In October, 1845, I published in Prof. Silliman's

"*Journal of Science*" an account of two such, one to exhibit sea-waves, the other sound-waves; in both of which the connection between the wave-propagation and the molecular motion is presented distinctly to the eye. The hint which led to the construction of these articles of apparatus was derived from a notice which I saw of Prof. Powell's instrument for showing the different varieties of polarization of light. It remained to contrive an article to present the vibrations of common or unpolarized light.

Now it is to be observed, that in sound the molecular vibrations are *longitudinal;* that is, the particles of the substance conveying the sound oscillate *in* the line in which the sound is propagated; while, in sea-waves, the vibrations are *transverse,* or the particles of water oscillate perpendicularly *across* the line in which the waves advance. As to light, it was at first supposed that the molecular vibrations are longitudinal, as in sound; and a strong argument in favor of this supposition was, that the relations of a ray of light to space around it are the same on every side. But both Young and Fresnel proved that longitudinal vibrations failed to explain some newly discovered phenomena of double refraction and polarization. The question then arises, How *can* the molecules oscillate transversely, without occasioning difference of properties on different sides? It can be answered only thus: the vibrations are made transversely in all directions, instead of being confined to one direction, as in sea-waves. And since they are repeated at the enormous rate of hundreds of millions of millions in a second of time, it is not *much* out of the way to say, that they are in all transverse directions at once.

The instrument before you is designed to illustrate this mode of vibration. To present the motion of a *series* of particles would scarcely be possible; hence the instrument exhibits the oscillation of one molecule alone. [The vibrations were then shown.]

The mechanism of the instrument is such as to cause the ivory disk (representing a molecule of the luminiferous ether) to describe an internal epicycloid,—the diameter of the revolving circle being very nearly, but not exactly, one half that of the circle revolved upon. It has long been known, that, if a circle revolve upon the interior of another circle of exactly twice its diameter, a point in the circumference of the former will describe a straight line, namely, the diameter of the latter; and if the generating point be placed either within or without the circumference, it will describe an ellipse, whose eccentricity will diminish as the point departs from the circumference,—becoming a circle when the

point reaches the centre of the moving circle, and also when it is at an infinite distance outside of it. Wheatstone's photometer affords an elegant illustration of these motions.

[These epicycloids were shown, by the use of an article of apparatus constructed for the purpose.]

To produce the particular motion I wished, it was necessary only to add or subtract a tooth in the circumference of either wheel. For, by this means, the place of contact between the two would shift at each revolution, making a slow progress round the circle, thus throwing the vibrations of the disk (attached to the small wheel) into all possible directions in regular succession. The distance of the molecule from the centre of the generating circle is adjusted at pleasure, so as to produce either rectilinear or elliptical vibrations,—the molecule being attached to the end of a slender rod, which can be slid backward or forward, and held in place by a spring. A rapid motion is produced by a pair of multiplying wheels; and the path of the molecule is easily traced, being projected on a ground of black ebony, which constitutes the entire face of the instrument, and conceals the mechanism from view. That the path of the molecule might fall on an unbroken surface, and thus be distinctly seen, it is necessary to allow the ebony plate to revolve with the axis of the generating circle.

[The mechanism of the instrument, and the different forms of vibrations produced, were then exhibited.]

According to Prof. Airy, this gradual progression of the ellipse of vibration cannot, in strict propriety, illustrate the molecular movement of common light. His proposition is this:

"Common light consists of successive series of elliptical vibrations (including in this term plane and circular vibrations), all the vibrations of each series being similar to each other, but the vibrations of one series having no relation to those of another. The number of vibrations in each series must amount to at least several hundreds; but the series must be so short that several hundred series enter the eye in every second of time."

He considers the truth of this proposition established by the fact, that in homogeneous light Newton's rings may be seen as far as to the *sixtieth* from the centre, showing that a complete interference occurs between a wave and the sixtieth following wave, as truly as between two successive waves; but there could not be a perfect interference, unless the first and the sixtieth waves were similar. My own observation has shown this interference to exist between the 1st and the 150th wave; as

the lenses which I use for the exhibition of Newton's rings give 150 distinct rings in the light of a monochromatic lamp.

If this is the correct view to be taken of the luminiferous undulations, then in the use of this instrument we have only to suppose the molecule to make a few millions of vibrations while its elliptic path lies in a particular direction, and a few millions more in the next position it takes, and so on. We are in no danger of multiplying the number too far in this way; for if the ellipse shifts entirely round in *only the* 1000*th part* of a second of time, it could stop on each *angular* second of its progress long enough for the molecule to make about 500,000 vibrations, before it passed to the next position, one second farther on. It would seem, that, by distributing the 600,000,000,000,000 vibrations per second in this manner, in accordance with Prof. Airy's idea, a sufficient number of positions would be occupied in a second of time, and a sufficient number of vibrations would succeed each other in each position, to meet the most delicate case of interference ever observed.

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2. Description of an Article of Apparatus for showing the Experiments of Complementary Colors in Vision. By Prof. E. S. Snell.

I beg leave to occupy the attention of the Section with a brief description of a simple article, by which a lecturer can enable his class, or any other audience, in a moderate sized room, to try all at once the experiment of producing the impression of complementary colors on the retina of the eye.

The instrument consists of two thin wooden disks, each about one foot in diameter, the front one being painted white and pierced with three apertures, three or four inches in diameter, and the other directly back of it, painted with six sectors, alternately white and colored. Any three bright and strongly contrasted colors will answer for the alternate sectors. The arrangement of the apertures is such, that in one position of the hinder disk, three colored spaces, e. g., red, yellow and blue, are seen, as on a white ground; and then, by a turn of 60°, the colors disappear, and the white sectors take their place. If, while the colors are in sight, the spectator fix his eyes on a small spot marked at the centre, till they grow weary, and the experimenter then turn the hinder disk, so as to render the whole surface white, the three spots at once seem to change their color to the complementary tints, delicate indeed, but quite distinct. Each of these dilute colors is more easily distinguished, be-

cause they are brought into contrast with each other, by being observed at one view. Thus blue is succeeded by buff color, yellow by violet, and red by green. The eyes should of course be kept fixed on the same point after the disk is turned, in order that the illusive colors may appear on the *same ground* as the real ones. It would be easy to have several disks prepared with different sets of colors, either of which could be attached or removed in a moment.

The article was exhibited, and the experiment performed.

These communications gave rise to remarks by Prof. W. B. Rogers, Prof. Henry, Prof. Peirce and Mr. G. P. Bond.

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3. Notes of a Discussion of Tidal Observations made in Connection with the Coast Survey, at Cat Island, in the Gulf of Mexico. By Prof. A. D. Bache, Superintendent.

In executing the hydrography of the entrance of Mobile Bay and of Mississippi Sound, connected tidal observations were made under the immediate direction of Lieut. Comd'g. C. P. Patterson, U. S. N., Assistant in the Coast Survey.

The results at Cat Island, at the entrance to Lake Borgne, Louisiana, and at Fort Morgan, at the entrance to Mobile Bay, have undergone more than one discussion, the peculiarities of the tides giving great interest to the observations.

The results, as obtained from a year's hourly observations day and night at Cat Island, will be given as far as obtained, the steps taken for further progress stated, and the information which has been obtained from other sources bearing upon this most interesting problem of the tides in the Gulf of Mexico will be briefly touched upon.

I hope, in the progress of the Survey along this part of our coast, to develope the subject of these tides, full of importance to the navigator, and of interest to the man of science.

These tides, with special exceptions, ebb and flow but once in twenty-four hours. The tide gauge was of a kind known as the box gauge, with a float and staff, graduated into feet and decimals of a foot. It was placed in the harbor of Cat Island, near the light-house, at the extremity of a temporary wharf.

The harbor, as the Coast Survey chart which I now present to the meeting shows, turns its widest and deepest opening to the East.

Apparent time was given by a mark, and the observations were made at mean solar time by applying the equation. The time was of less consequence than ordinary in these observations, from the small rise and fall of the tide, which prevented small differences of time from being noticeable by differences of rise and fall. Slight inequalities, caused chiefly by wind, were also found to affect the observations so materially that it was not deemed advisable to observe oftener than once the hour; and after attempting to determine the epoch of high and low water by more frequent observations, it was determined that errors would probably be introduced by aiming at a degree of precision which the phenomena themselves did not present.

The observations were made day and night, hourly, for a year, with exceedingly rare omissions, and, as the discussion has shown, with a degree of faithfulness which merits very great praise. The observers were Messrs. GUSTAVUS WURDEMAN and R. T. BASSETT, attached to the Coast Survey.

The general opinion of nautical men on the subject of these tides is, that they mainly depend upon the action of the wind; and the very regular effect which may be shown to result from a discussion of the tides in reference to the local action by the wind, lends plausibility to this generalization, which nevertheless is unfounded.

The causes are of a much more general character, and such as usually influence the tides, so modified as to be difficult to bring out; phenomena which are only accessory in the ordinary discussions assuming here the chief and overruling part.

The regular tabulation of the observations was made by Lieutenant Comd'g. C. P. PATTERSON, who did not fail to perceive that the ordinary methods of discussion of the tides were inapplicable. His removal from the Survey on other professional service has devolved upon me the labor of discussing the results.

Their importance, interest, and novelty, so far as our coast and their striking peculiarities are concerned, have justified me in giving much time to the discussion, which has been carried on under my immediate direction, by Mr. G. W. DEAN, Sub-Assistant in the Coast Survey, and by Messrs. R. M. BACHE, A. S. WADSWORTH, Jr., and W. M. JOHNSON.

I am indebted for the diagrams necessary to illustrate the conclusions already arrived at, to Messrs. BACHE, JOHNSON, and KEYSER.

I present a part only of the labors of these gentlemen. The whole of the hourly observations for the year have been thrown in the form of

curves, and numerous tables for examining and verifying the different hypotheses have been made by them. Though the subject was reached inductively, I do not propose to present it strictly in that form.

The work even now is far from being complete; indeed we have rather reached the true method of discussion, than have completed the discussion; and we may yet have to modify our hypothesis, though I think not materially. I present it to the Association as a work *in progress.* When the investigation for this Section is made complete, the application of the methods to the other stations on the Gulf of Mexico will be in a degree mechanical.

It is curious that one among the earliest complete series of tidal observations on record, is of tides ebbing and flowing but once in twenty-four hours. The observations were made by Mr. Francis Davenport, at Batsha, of the tides on the bar of Tonquin, and communicated to Dr. Halley, who gave them, with a diagram connecting the phenomena with the moon's motion in the ecliptic, in the thirteenth volume of the Philosophical Transactions for the year 1683. Newton explained these tides by his lunar theory, but in a way, as appears to me, to leave it doubtful whether he supposed the interference of two ordinary or six hour tides to produce the phenomena. These tides have been referred to since by almost every writer of note who has given a general theory of the tides.

The subject of the diurnal inequality of the tides has been so completely and ingeniously discussed by Mr. Whewell, Master of Trinity, that it may be said emphatically to be his own. He first pointed out the empirical law of variation of this inequality. The first distinct attempt to trace the cause of apparent ebb and flow once in twenty-four hours to the influence of the diurnal irregularity, is also, as far as I know, his. In discussing (Philos. Trans. for 1837, Part I.) the tides at Singapore, where the diurnal inequality is very large, he was led to the conclusion, if carried a little further, "at a certain stage of it the alternate tides would vanish." To this effect he attributed the "single day tides of King Georges's Sound, on the coast of New Holland, as observed by Captain Fitz Roy," and gives the curves for a week's observations on the diagram accompanying his papers. The progress of the diurnal inequality wave along the coast of Europe forms an interesting part of Mr. Whewell's labors, the conclusions of which are given in the same volume of the Philos. Trans.

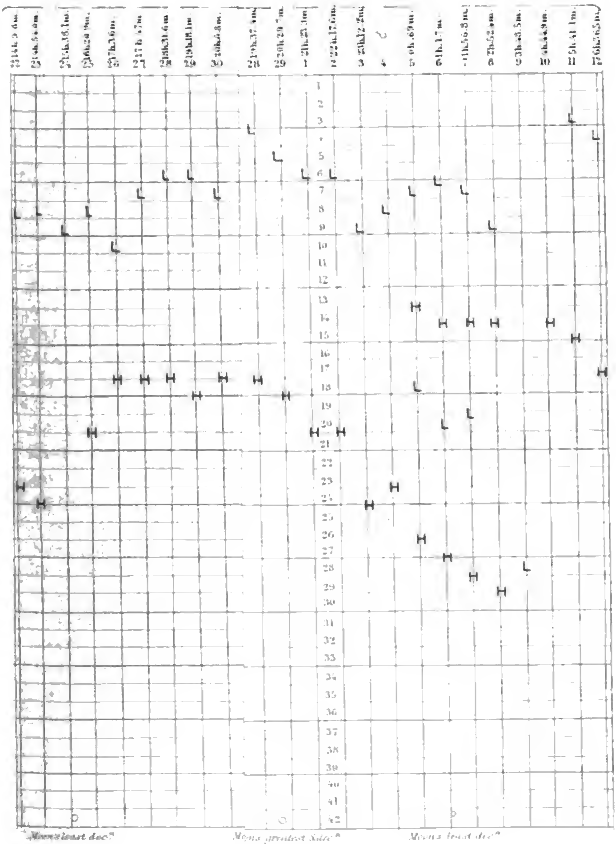
In all these cases, however, there are two tides in the course of the day, so as to bring out the diurnal inequality by the comparison of the

consecutive high or low water. The subject is followed up in the eleventh series of tidal researches by Mr. Whewell, and in the appendix, in which the diagrams of the tides of Petropaulofsk, in the Bay of Avatcha, Kamtschatka, approaching very nearly, at certain parts of the lunar month, to the order of single day tides, is given, to prove that the diurnal inequality may be so large "as to lead to the appearance of only one tide in twenty-four (lunar) hours." The equations of the diurnal and semi-diurnal tide waves are given in this paper, and the wave produced by certain cases of their interference is discussed. (Philos. Trans. for 1840.)

I do not pretend to give such notice of these important papers as would be necessary in a formal communication. Unquestionably the observations now under examination would have furnished to Mr. Whewell only the means of trying ideas and consequences flowing from those which have been already discussed by him, yet the forms of discussion are original, and perhaps new, and the conclusions present so much of novelty that they remain to be fully put to the test by more elaborate discussion, and by bringing the results at other places to bear upon the same question. I am forced by the necessity for brevity to omit a reference to the learned, ingenious, and elaborate paper of Mr. Airy, in the Philos. Trans. for 1848.

The small rise and fall of the tides, amounting on the average to but one foot, would seem to make it difficult to obtain the law of the phenomena, even with the aid of the most careful and truthful observations, the class to which those under discussion have proved to belong. In regard both to time and height, we may expect to be baffled by small irregularities, requiring long continuance of observations and comparisons of means, to get rid of. Thus far, few cases have occurred which do not exhibit more striking coincidences than differences.

1. To show the time of high or low water in such a way that the discussion might be readily generalized, the diagrams, of which a specimen is before the Association, were made; the hours of the day are the ordinates, and the days of the month the abscissa. The signs H and L show in their proper place the hour of occurrence of high and low water for each day. The time of the moon's superior transit is marked, and the periods of greatest declination, and of crossing the equator. The result is easily generalized, that there is ordinarily but one high and one low water at Cat Island in twenty-four (lunar) hours, and that when there are two tides they occur about the time of the moon's crossing the equator, and are usually most regular and strongly marked



when in syzigies, with declination nearly zero. Following one set of high and low waters, it will be found that they occur later and later as the lunar day gains on the solar, with very remarkable differences, of which the explanation will be given, towards the period of small declinations. The interval from high to low water is generally less by some hours than that from low to high. That as the moon approaches the equator, there are a few days of singular double tides, or of single tides, in which the times from low to high water are very much increased. That when the declination changes its name, a high tide takes nearly the place of a low in time, and *vice versa*, with an interval of irregularity, or, in other words, the tides are displaced by nearly twelve hours.

2. There is, as Mr. Whewell has remarked, no proper establishment to be derived for such tides; yet we may obtain a desirable datum by throwing the results into the form of tables, in which the luni-tidal intervals are arranged according to the days from the zero of declination and the corresponding superior and inferior transits, and for N. and S. declinations. This will be made more clear by subsequent explanation. These afford a test of the theory of these tides, by showing the displacement of the ordinate of high and low water, and might be used for the inverse purpose of forming prediction tables. Such tables of luni-tidal intervals for three months I now submit. They show considerable steadiness and similarity of intervals towards the maximum of declinations, and great variations near the zero, and greater discrepancies than is usual in ordinary tides. These are from a series of tables computed by Mr. R. M. Bache for the year, and containing the times of high and low water, deduced from the daily curves, the readings of the gauge, the rise and fall of the tides, the times of the moon's superior and inferior transit, and the moon's declination.

The intervals serve to show that the high water belongs alternately to the superior and inferior transits of the moon, according as the moon's declination is north or south, with a few cases only which admit of doubt. Two sets of luni-tidal intervals were computed (see tables) for three months, to ascertain the proper epoch of reduction (or age of the tide). In one case the intervals were referred to the superior transit of one day before, and in the other to the superior transit of two days before. The square of the discrepancy of the mean in the latter case was greater than in the former. An establishment deduced from these numbers for high water without correction would have a probable error, as tried by discrepancy from the mean, of nearly

eighty-four minutes. I have little doubt of being able to reduce this error, by computation, much within the limits of observation, so as to give useful prediction tables. The foregoing results point distinctly to a ruling cause depending upon the moon's declination.

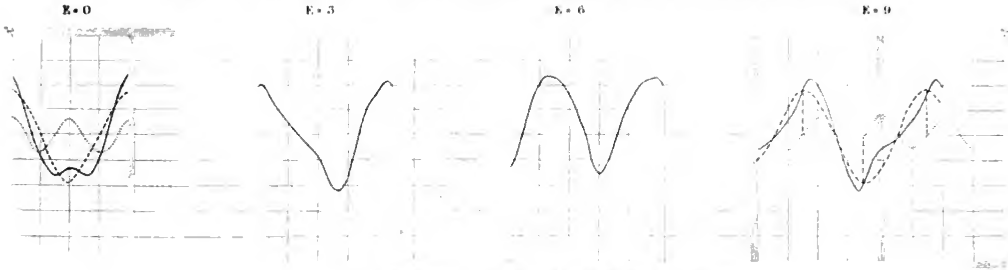
3. The hourly observations for the year were thrown into the form of curves, the abscissas representing the hours and the ordinates the heights. Of these I present, as characteristic, the months of January and March. In January the tides are single throughout the month, the rise and fall diminishing towards the zero of declination; and in March two periods of marked double tides occur. The times of new and full moon coincide nearly with the zero of declination of March; in January the syzigies occur at times of greatest declination. A series of diagrams, prepared for periods of declination zero, shows irregularities or double tides near these times, and those for periods of greatest declination regular single tides. Before disappearing, the tide which is last appears rather as an irregularity than as a real tide, puzzling to the observer and a severe test of his faithfulness. A similar set of diagrams for the periods of greatest declination show uniformly single tides and the greatest comparative rise and fall at the same periods, whether coinciding with syzigies or with first and last quarters. In computing the heights of spring and neap tides by the common methods, four months gave zero or negative differences.

To discuss the epochs of the phenomenon, as compared with greatest and least declinations, I prepared two sets of tables, which require revision.

They show sometimes an actual coincidence in the epoch of lost tides and zero of declination, sometimes a precedence and sometimes a subsequence, which, when not caused by irregularity of winds, I believe will find a satisfactory explanation; at a mean, there was little advantage in the discussion found from displacing the epoch. The average rise and fall for the second day before the greatest declination was 1·68 ft., for the day next preceding the greatest declination 1·78, for the day of greatest declination 1·81, for the next day 1·86, and for the next 1·77. Tracing a curve from these would give the epoch of greatest rise and fall about 0·75 days after the greatest declination. The average rise and fall on the corresponding days, in reference to declination zero, were 0·96 ft., 0·75, 0·60, (dec. zero,) 0·63, 0·73, the curve giving the epoch about one-sixth of a day after the zero of declination. The numbers, as stated, require revision; and there are causes for an apparent displacement, which require further examination.

No. 4.

Forms of curves by interference for same values of C. & D. & E. for 0. 3. 6. & 9. hrs.



The space between the large vertical lines represents one day
" " " " small " " " 6 hours
" " " " " horizontal " " half a foot

4. This general examination tends to point to the diurnal irregularity, as Mr. Whewell has stated, as the cause of the occurrence of these single day-tides; a view which is confirmed by such examinations as I have been able to make of the hourly tidal observations at Fort Morgan, at the entrance of Mobile Bay. The interference in this case would be between the diurnal tide-wave, which represents the diurnal inequality and the ordinary semi-diurnal wave, whether this wave has a regular progress along the coast independently of the semi-diurnal wave, as was at first supposed by Mr. Whewell, or whether its phenomena are local, as he has since been led from his investigations to believe. If the observed wave is produced by its interference with a semi-diurnal wave, we can only study the phenomena to advantage after the observed wave has been separated into its components.

5. As a first approximation, I assumed the two waves to be governed by the law of sines, and then determined the curve which would result from the superposition of two such waves, having the same or different origins. The mean of the regular double tides about the zero of declination would present a first approximate value of the rise and fall of the semi-diurnal tides, and the mean of double and single tides at the maximum of declination would, especially when near the quadratures, give a first approximation to the height of the diurnal tide. The comparisons with the forms of curves already traced, addressing the eye, are easily made.

I present herewith diagrams for the case, in which the maximum of the diurnal tide coincides with that of the semi-diurnal, is three hours in advance, (or coincides with mean water falling,) six hours, (or coincides with low water,) and nine hours, (or coincides with the second mean, or mean water rising,) using the approximate quantities referred to above for the greatest height of two component curves. It requires little examination to see that neither of the first three forms represents the case, and that the fourth does so remarkably, even in what appear to be small irregularities in the daily curves. This will be seen in the results for October, of which a diagram on a large scale is presented, giving the tidal curves near the zero, and thence up to the maximum of declination for the first half of the month. In the single day tides there was the same slow rise compared with fall, sharp rise and fall near high and low water, with the tendency to a stand during the rise; the same excess in the interval of time from low to high water, over that from high to low water. This hypothesis as to the position of the two waves may perhaps be slightly improved by further discussion. It is obvious

from the equation of the curve (which I have already referred to as given by Mr. Whewell), that the form and position of remarkable points will vary with the constants in the component curves, as well as with the position of the origin of each in reference to that of the other.

To carry out the representation graphically, I have drawn the curves for four values of the constants of the diurnal and semi-diurnal, formed from the observations with the same displacement of nine hours in the time of high water of the diurnal curve, and corresponding to the epochs of the maximum declination, two, four, and six days before or after the maximum. These show the general features of the curve sufficiently, and the variations in the times and heights, the passage from single to double tides, and the reverse; and the coincidence with observations is such as to warrant a close numerical discussion.

6. The equation of the curve shows how the time of high and low water depends on the constants in the diurnal and semi-diurnal curve.

The equivalent of the equation given by Mr. Whewell is—

$$C \,.\, \cos 2t + D \cos (t - E) - y = 0,$$

in which t is the time in hours from the places of the maximum ordinates of the semi-diurnal curve as an origin. C is the constant of that curve of sines; E is the distance of the maximum ordinate of the diurnal curve for the former, and D the constant for the curve of sines; y is the ordinate of the complex curve.

By an easy transformation, this takes the form,

$$\cos^2 t \left(2C - \frac{D}{2} \sin E\right) + \cos t \,.\, D \,.\, \cos E + \sin t \,.\, D \sin E - C - y = 0,$$

or, for our present purpose,

$$2C \,.\, \cos^2 t + D \cos t \,.\, \cos E + D \,.\, \sin t \,.\, \sin E - C = y.$$

For E = 9 hrs. $\operatorname{Cos} E = \sin E = \sqrt{\tfrac{1}{2}}$,

and $y = 2C \cos^2 t + D \sin E (\sin t - \cos t) - C$.

The differential co-efficient of which for the case of the maximum or minimum is

$$\frac{dy}{dt} = -4C \cos t \,.\, \sin t + D \sin E (\sin t + \cos .\, t) = 0$$

$$\frac{1}{\sin t} + \frac{1}{\cos t} = \frac{4C}{D \sin E} = \frac{4C}{D \sqrt{\tfrac{1}{2}}},$$

or, since the second term is negative when $t > 6$ hours,

$$\text{cosec}\,.\,t - \sec t = \frac{4C}{D\sqrt{\frac{1}{2}}}.$$

Applying this to the four cases shown in the diagrams

				hrs.	min.
$E = 9$ hours, $C = 0{\cdot}175$,	$D = 0{\cdot}700$	we find	maximum at	10	25·4
	$= 0{\cdot}615$			10	33·3
	$= 0{\cdot}400$			10	51·1
	$= 0{\cdot}157$			11	56·8

and for the intervals between high and low water in lunar hours, 9 h. 09·2 m., 8 h. 53·4 m., 8 h. 17·8 m., and 6 h. 06·4 m.

We might apply this mode to test the hypothesis, using for the values of C, the half difference of the ordinates of six and twelve hours from the mean, and of eighteen and twenty-four hours with the signs changed, and for D, the average of the ordinates of six and eighteen hours from the first mean. The means present the best criterion because not displaced in this combination as the equation shows. This mode of proceeding, however, throws the test too much on the weak part of the results, the times of occurrence of high and low water, or of mean water, and do not take in all the points of the curve, and I have therefore preferred a different form of discussion.

7. Placing the maximum of the semi-diurnal curve at 0 hours, in the hypothesis that the high water of the diurnal curve is nine hours in advance of that of the semi-diurnal curve, the two curves cross the line of mean water at three hours, the diurnal curve rising and the semi-diurnal falling; at six hours, the semi-diurnal curve has reached its maximum, and rises again at nine hours to its intersection with the mean water line, at which time the diurnal curve has reached its maximum; the semi-diurnal curve attains its greatest rise at twelve hours, and the mean level at fifteen; the diurnal curve also descending to the same point at that time.

Within these two intervals from mean level to mean level, the combinations of the ordinates forming the actual tidal curve are exhausted; the part of the curve below the mean level being symmetrical with that above. From three to nine hours, the ordinates of the semi-diurnal curve are subtractive; from nine to fifteen hours, additive. The mean is the average between high and low water. The tides of each day will give

theforms of the component curves, beginning with the mean, and ending with it, considering as symmetrical the parts above and below the axis of X.

In tabulating, the branch above the axis should be referred to the mean of the preceding and succeeding low water $\left\{ \frac{l+l'}{4} + \frac{h}{2} \right\}$ and of the high water which it includes, and that below to the mean of the two high, and of one low water. From three to nine hours, the difference of the ordinates giving the actual curve, and from fifteen to nine in the reverse order the sum of the same ordinates, half the sum of the two series of ordinates gives the value of the ordinates of the diurnal curve, and half the difference, the ordinates of the semi-diurnal curve. The same being repeated with the second branch of the curve, the average will give two results for each day's observation.

The case given in the table on the boards, for March 5, will serve to illustrate the simple nature of this method of proceeding.

The mean ordinate for the first and second branches of the curve having been obtained, and the hourly observation which coincides most nearly with it having been found before and after high water, the hourly observations are arranged from it forwards for seven hours (m) and backwards for seven (n). The same is done for low water, (m′ and n′.) The half sums and half differences are taken in each case, and then the means. The computation of the diurnal curve is made in the upper part of the table, and that of the semi-diurnal curve in the lower part. The number representing the mean level is eliminated by the mode of taking the means in each table, and the ordinates below the axis are treated as if having the same sign as those above. The semi-diurnal curve is turned over on its maximum ordinate, and the mean value of a single branch of it found. Then each curve is reduced to zero, in the mean level of the period. The last two columns of the upper and lower part of the table contain respectively the curves of sines corresponding to the diurnal and semi-diurnal curves.

In the case shown in the first diagram, the ordinates of the semi-diurnal curve from mean water to high water, and corresponding nearly to a minimum of declination, and new moon, are 0·00 feet, +0·02, +0·03, +0·05, +0·04, —0·02, +0·02. The moon's declination during the period being about from 2° 54′ S., to 1° 45′ S.; this curve obviously contains a residual of the semi-diurnal curve, not taken out, but supposing it to be deduced from a just mean, the corresponding ordinates of a semi-diurnal curve, calculated with 0·04 feet as the maximum would be

0·00 feet, 0·01, 0·02, 0·03, 0·03, 0·04, 0·04, differing at the most, 0·06 of a foot, or about three quarters of an inch, and in a single instance, the sum of all the six differences being 03 feet, and the average ·004.

The ordinates of the semi-diurnal curve are 0·00 feet, 0·14, 0·28, 0·32. The curve of series computed with the greatest ordinate has in this case for its corresponding ordinates 0·00 feet, 0·16, 0·28, 0·32, differing but ·02 feet at the greatest.

At the next period of declination nearly zero, and full moon in the month of March, the ordinates of the diurnal curve deduced are 0·00 feet, 0 05, 0·06, 0 06, 0·08, 0·06, 0·09, and the corresponding computed ordinates 0·00 feet, 0·02, 0 04, 0·06, 0·07, 0 09, 0·09, differing at the greatest 0·03 feet, and on the average, 0·004 feet, the observed being this time in excess as it was before in defect. The ordinates of the semi-diurnal curve are 0·00 feet, 0·12, 0·22, 0·26, and the computed ones, 0·00 feet, 0.13, 0·24, 0·26, the greatest difference being 0·02 feet, and the average 0 007 feet in excess, as was the former.

For March 12, corresponding to the maximum of the diurnal curves, and to neap tides (one day after last quarter), the ordinates of the diurnal curve hourly from mean to high water are 0·00 feet, 0·21, 0·36, 0·51, 0 63, 0·69, 0 71, the corresponding ordinates of the curve of sines being 0 00 feet, 0·18, 0·35, 0·63, 0 69, 0·71, in which the greatest difference is 0·03 feet, and the mean +0·007 in the curve computed from observation. The ordinates of the semi-diurnal curve are each zero. Two days afterwards, viz. March 13, gives for the diurnal curve, 0·00 feet, 0·18, 0·34, 0·47, 0·61, 0·68, 0·74, corresponding to which is the curve of sines 0 00 feet, 0·18, 0·37, 0·51, 0·63, 0·72, 0·74, in which the greatest difference is 0·04 feet, and the mean—0·02 feet, the curve of observation having the least ordinates. The semi-diurnal curve is 0·00 feet, 0·00, 0·03, 0,02.

The average of three months, taken by weeks, gives for the mean curve and curve of sines the following table:—

Hours.	Diurnal Curve.			Semi-Diurnal Curve.	
	From Observat'n. Feet.	Of Sines. Feet.	Diff.	From Observation Feet.	Of Sines.
0	0·00	0·00		0·00	0·00
1	0·17	0·15	0·02	0·04	0·04
2	·32	0·30	0·2	0·07	0·07
3	0·43	0·42	0· 1	0·08	0·08
4	0·52	0·52	0·00		
5	0·56	0·58	— ·02		
6	0·58	0·60	—0·02		
		Sum	0·01		

These results are shown by a curve in the diagram herewith presented (Pl. 5) on the full scale,* the greatest difference between the curve from the observation and the curve of sines being less than a quarter of an inch in the mean deduced from three months observations. Whether this will disappear in the mean of more observations or whether a modification of the hypothesis of displacement of nine hours must be made to meet it, further computations now in progress will show.

8. When this analysis has been made as complete as possible, and applied to the year's observations, it will remain to take up the two series into which we have divided the observations, and to discuss these numerically in detail, as we have heretofore generally done, in regard to the known laws of the diurnal irregularity, and of the ordinary tides. Each determination gives a corresponding value of the maximum, or of the ordinate of high water, and in the case of the mean of the curves for January, February, and March, these maxima are 0·66 feet, 0·65, 0·60, 0·60, 0·58, 0·58. Mean 0·61 feet, differing ·03 of a foot, from the maximum found directly from the observations, and if the discrepancies are accidental, giving a mean probable error by the variations from the average of 0·02 feet (one quarter of an inch) for any one of the determinations, and for the mean, 0·01 feet nearly.

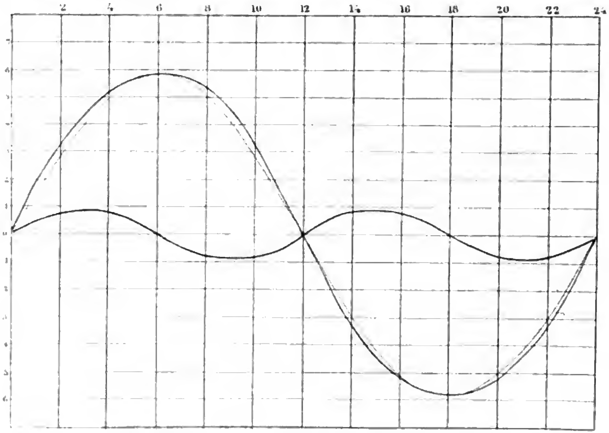
9. By the kindness of Col. Abert, of the Topographical Engineers, of Major Bache, of the same corps, and of Lieut. Maury, Superintendent of the National Observatory, I have been put in possession of tidal registers which have been kept during the progress of the local surveys made of harbors on the coast of the Gulf of Mexico. The tidal observations of Major Bache, U. S. Topographical Engineers, at Key West and the Tortugas, are the most complete of this series, and show, as a general phenomenon, the prevalence of the semi-diurnal wave at that point. I have not yet had the opportunity to examine fully these results, which are however under discussion.

At the close of this communication, Capt. Wilkes inquired of Prof. B. as to the direction of the diurnal and semi-diurnal tides. Prof. Peirce made some remarks on Laplace's investigation of the theory of the tides, and on the imperfection of our knowledge of the subject, due to the great modifications caused by the bottoms and shores of seas, and other causes. Lieut. Davis inquired respecting the epochs of the diurnal and semi-diurnal inequality. Prof. S. Alexander referred to Prof. Bache's results as furnishing an impressive example on the utility of the *graphical method* in physical inquiries.

* Reduced in the plates.

No. 5

Mean curves for Jan. Feb. and Mar. deduced from observations & computations.



The space between the vertical lines represents 2 hours.

" " " horizontal " " $\frac{1}{10}$ of a foot.

4. On the Use of Wood in the Construction of Bridges and Roofs of Large Span in America. By Charles Culmann.

Mr. Culmann described in chronological order the principal forms of these structures which have been devised, closing with the light and stretched wooden bridge of Remington. He pointed out in each case the mode in which the chief parts of the frame-work acted in distributing the tensile and compressing forces, and gave a formula for computing the value of these forces in certain cases.

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5. On Monsoons on the Shores of the North Atlantic. By Prof. J. H. Coffin.

Prof. C. exhibited diagrams showing the mean direction of the wind for an entire year at numerous localities. He showed a disturbing force at places in the neighborhood of the ocean, by which the tendency of the wind in the winter is towards the ocean, and in summer towards the land.

FIFTH DAY, FRIDAY, AUGUST 23, 1850.

SECTION OF PHYSICS AND MATHEMATICS.

(*Afternoon Session.*)

The following communications were presented:

1. On Optical Moving Figures. By Prof. E. Loomis.

Prof. L. said, there is an amusing optical experiment frequently exhibited by means of a small figure—a mouse, for instance—worked in red worsted upon a green ground, or worked in green upon a red ground. When this figure is moderately illuminated by the light of a candle and gently agitated, the mouse appears as if in motion upon the canvass. Having met with no explanation of this experiment which is entirely satisfactory to my mind, I have instituted a series of new

experiments, in the hope of deducing from them a plausible theory. The following are a few of the experiments:—

First. If we place a red circle of half an inch diameter upon a green ground, moderately illuminated by the light of a candle, after directing the axis of the eye steadily upon the circle for a few seconds, it appears of a paler red than ordinary; but if we direct the axis of the eye a little to one side of the circle, we see it of a deeper red than ordinary. As often as the axis of the eye returns to the circle, it appears pale; the moment it is thrown off, the circle appears of a very deep red.

Second. If we place a green circle upon a red ground, when the axis of the eye is directed upon it, the circle appears of a darker green than ordinary; but if the axis be turned a little to one side, it appears of a brighter green than ordinary. The dark green and the light green may be made to succeed each other at pleasure, by a slight motion of the axis of the eye. If the axis of the eye be thrown a little above the circle, it appears light; if it be directed fairly upon the circle, it appears of a dark green; if the axis be thrown a little below the circle, it appears light.

Third. If the axis of the eye be kept fixed in one position, and the circle be moved up and down, then when the circle coincides with the direction of the axis, it appears of a dark green; but in every other position, it appears of a light green. An oscillatory motion of the circle, therefore, causes the appearance of a dark shade of green passing over it.

Fourth. When the last experiment is repeated with a red circle upon a green ground, the appearance is that of a light shade of red passing over it.

Fifth. If instead of a circle we employ a figure of a small animal, when the card is agitated the appearance is that of a shadow passing over it, and we receive the impression of an animal in motion.

Sixth. If a blue color be substituted for green, in each of the above experiments, the effects are nearly the same. Indeed, certain shades of blue and green cannot be distinguished from each other by candle-light. Number one is thought to be the fundamental experiment from which all the others naturally follow. This experiment appears to be tolerably well explained, by supposimg that the "foramen centrale" is less sensitive to red light than other parts of the retina. Accordingly, when the axis of the eye is turned directly upon the red circle, it appears dull; but if the axis be thrown a little to one side, the red

appears much deeper. When the axis of the eye is turned upon the green circle, it appears darker—that is, less vivid; but when the axis is thrown a little to one side, the green appears more brilliant.

Seventh. These experiments will only succeed with certain combinations of colors. Red must be combined with green, or blue, or purple. Orange must be combined with light blue; yellow with a very light blue, green with red, blue with red, violet with red. Considering that none of the colors used in these experiments are the pure colors of the spectrum, it is thought that in each of these combinations one of the colors may be pronounced complementary to the other. The reason why complementary colors must be associated, probably, is, that when the eye is strongly impressed with any color, it becomes particularly sensitive to its complementary color. Hence, although we may obtain similar results with a green circle upon a white ground, the changes of tint are but slight, and would not be noticed unless particular attention were given to the experiment. The changes are most striking when a green circle is placed upon a red ground.

Why do not these experiments succeed equally well in daylight? The eye appears to be most sensitive to slight changes of shade when it is not excited by a very strong light. Hence these experiments succeed best with a moderate illumination. They do not succeed well in a strong light, whether it be gaslight or daylight. The eye is most sensitive when screened from the direct effect of the light, and these experiments succeed best when the back is turned towards the source of light. The experiments, therefore, succeed but indifferently when the light comes from a great number of candles in different parts of the room, or from the diffuse light of day. The effect of the complementary color, in deepening the tint of any object, may be shown by placing a stripe of green, half an inch in breadth, upon a red ground. The centre of the stripe appears much lighter than the margin.

Professor Loomis exhibited specimens of the various colors worked in worsted on cards.

Some conversation ensued between Prof. A. D. Bache and Prof. Loomis. They had both failed to obtain the effect with paper, colored to resemble the worsted work, and Prof. Bache expressed the opinion that the *pile* of this species of fabric was essentially connected with the phenomena.

2. On the Influence of Temperature in the Absorption of Light. By Wolcott Gibbs, M. D., Professor of Chemistry, Free Academy, New York.

In a memoir on the production of Light by Heat, Dr. Draper has endeavored to show that when any substance is heated so as to become luminous, the refrangibility of the luminous rays, or, more strictly speaking, the proportion of the more refrangible rays, increases with the temperature. The observation of Dr. Draper led me to study the order of succession of the colors which are developed in certain chemical compounds by simple changes of temperature, unaccompanied by chemical or by permanent physical change; and I believe that I have succeeded in establishing a principle which is not unworthy of notice, when we consider the great imperfection of our knowledge, both of the theory and of the phenomena of the absorption of light. This principle may be stated as follows:—In the case of those substances which undergo changes of color upon the application of heat, the refrangibility of the reflected rays diminishes with the increase of the temperature, or otherwise. The absorption of light, consequent upon the elevation of temperature, proceeds from the violet towards the red end of the spectrum. In support of this statement, I here give a list of substances, together with the changes of color which they undergo when heated. For this list, I am indebted chiefly to Gmelin's Handbuch der Chemie.

Hyponitric acid,	colorless	pale-yellow, orange-red.
Titanic "	white	lemon-yellow.
Tantalic "	"	"
Niobic "	"	"
Pelopic "	"	"
Molybdic "	"	"
Peroxide of tin,	"	orange-yellow.
Oxide of zinc,	"	lemon-yellow.
Peroxide of antimony,	"	"
Oxide of antimony,	"	"
Antimonic acid,	pale-yellow	brownish-yellow.
Tungstic acid,	lemon-yellow	orange-yellow.
Chromic acid,	orange	red.
Chromate of potash,	lemon-yellow	aurora-red.
" soda,	"	"
Bisulphide of arsenic,	orange	red-brown.
Oxide of bismuth,	lemon-yellow	orange, reddish-brown.

Sulphide of cadmium,	"	brownish, kermes-red.
Oxide of lead,	yellow	brownish-red.
Chromate of lead,	"	"
Peroxide of mercury,	brick-red	blood-red, brownish-black.
Cinnabar,	scarlet	carmine.
Sulphate of mercury,	white	yellow, red.
Basic nitrate of do.,	yellow	red.
Oxybromide of lead,	"	lemon-yellow, reddish-yell'w.
Iodide of lead,	lemon-yellow	reddish-yellow, brick-red.
Iodide of silver,	yellow	orange-yellow.

It will be seen that this list contains nearly thirty different substances of various constitutions, and varying in color at ordinary temperatures. In all these cases, the principle stated holds good. It is proper also to mention, that in all these cases cited the change in color is strictly dependent on the increase of temperature; and that the colors succeed each other in the inverse order when the different substances mentioned are allowed to cool, the body finally regaining its original color.

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3. On a Mode of effecting the Achromatism of the Telescope, by Lenses of the same dispersive Power. By G. P. Bond, Cambridge.

[*Not received.*]

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4. Notes on the Progress of the Determination of Difference of Longitude between Greenwich and Cambridge, for the Coast Survey, by W. C. Bond, Director of the Cambridge Observatory. By G. P. Bond, Cambridge.

Mr. Bond stated that, in the absence of his father from the meeting, he had been requested by the Superintendent of the Coast Survey to mention some of the methods employed in the expedition, and the results thus far attained.

The expedition was undertaken by Mr. W. C. Bond, the Director of the Observatory at Cambridge, at the expense of the Coast Survey. The chronometers were transported between Boston and Liverpool by the Cunard line of steamers. The first voyage, with forty-seven chronometers, was made in August, 1849. Fourteen voyages with

sixty chronometers, have been computed, giving a longitude of the observatory at Cambridge $4^{h.}\ 44^{m.}\ 30^{s.}1$ west of Greenwich.

This result is not regarded as final. It is proposed to prosecute the enterprise with such improvements as have been suggested by the trials already made.

FIFTH DAY, FRIDAY, AUGUST 23, 1850.

SECTION OF CHEMISTRY AND MINERALOGY.

(*Morning Session.*)

The Section met at 10 A. M. in its usual place. Prof. W. R. Johnson having been called to the chair, and Dr. R. E. Rogers as Secretary, the following communications were made .—

1. On the Absorption of Carbonic Acid by Acids and Saline Solutions. By Profs. W. B. Rogers and R. E. Rogers, of the University of Virginia.

In this communication we propose to embody the results of a series of experiments on the absorption of carbonic acid, begun some years ago, and of which the first part has already been published in Vol. VI. of the *American Journal of Science*. As that paper contains a minute description and a drawing of the apparatus used in our experiments, together with an account of the various precautions and corrections adopted to secure accurate results, we may dispense in this place with any description of our process, referring for such details to our previous communication.

The following table embraces all the results of our experiments made at the temperature of 60°, including those with water. Our former paper gives the detail of numerous observations of the absorption of carbonic acid by this liquid at other temperatures, from 32 as high as 212. The numbers in the table are the mean, in most cases, of four experiments, and in no instance of less than two. So uniform was the action, and so consistent the indications of our apparatus, that the successive experiments with the same substance rarely differed as much as one per cent. of the volume absorbed, and in no instance was as great as two per cent.

Table of the volumes of Carbonic Acid absorbed by one volume of various liquids at 60° F.

Water,		1·005
Sulphuric Acid,	S. G. 1·838,	0·940
Nordhausen Acid,		1·255
Hydrochloric Acid,	S. G. 1·205,	0·530
Nitric Acid,	S. G. 1·366,	1·245
Acetic Acid,	S. G. 1·077,	1·042
Oxalic Acid,	Saturated sol.	0·970
Tartaric Acid,	" "	0·553
Citric Acid,	" "	0·590
Chloride of Sodium,	" "	0·255
Chloride of Calcium,	" "	0·097
Chloride of Magnesium,	" "	0·352
Chloride of Barium,	" "	0·473
Chloride of Potassium,	" "	0·605
Chloride of Ammonium,	" "	0·775
Sulphate of Soda,	" "	0·485
Sulphate of Potassa,	" "	0·705
Sulphate of Magnesia,	" "	0·153
Potash Alum,	" "	0·763
Protosulphate of Iron,	" "	0·345
Sulphate of Copper,	" "	0·510
Sulphate of Zinc,	" "	0·323
Nitrate of Ammonia,	" "	0·540
Nitrate of Potassa,	" "	0·748
Nitrate of Baryta,	" "	0·882
Chlorate of Potassa,	" "	0·870
Yellow Prussiate of Potassa,	" "	0·615
Bichromate of Potassa,	" "	0·855
Bitartrate of Potassa,	" "	0·990
Acetate of Soda,	" "	1·370
Phosphate of Soda,	" "	0·385
Oxalate of Ammonia,	" "	0·905
Bi-cyanide of Mercury,	" "	0·940

To facilitate the comparison of our results with those of Saussure, we add the following table, including all the acids and saline solutions of Saussure's list, except the nitrate of soda, on which we have made no experiments :—

	S.	R.
Sulphuric Acid,	0·45	0·94
Tartaric Acid,	0·41	0·55
Chloride of Sodium,	0·329	0·255
Chloride of Calcium,	0·261	0·097

	S.	R.
Chloride of Potassium,	0·61	0·60
Chloride of Ammonium,	0·75	0·77
Sulphate of Potassa,	0·62	0·70
Potash Alum,	0·70	0·76
Nitrate of Potassa,	0·57	0·74

Except in the case of sulphuric acid, nordhausen acid, hydrochloric acid, and nitric acid, the carbonic acid gas submitted to absorption was not previously dried, but, as shown in our former paper, was in a state of saturation with aqueous vapor when brought in contact with the absorbing liquid. By applying the formula $v = V. \frac{p-f}{p}$, the volume of moist gas which disappeared might readily be reduced to dry gas, and with this view, observations of the barometer were made at the time of each experiment. As, however, the ratio $\frac{p-f}{p}$ which represents the proportion of the dry to the moist gas, is also the proportion of the gaseous pressure in the two cases, it will follow, according to the law of Dr. Henry, that the volume of dry gas absorbed under the full pressure of the dry gas, must be equal to the volume of moist gas which is absorbed under the reduced pressure due to the mixture of aqueous vapor. Hence, the numbers obtained in the table represent truly the volume of dry gas absorbed by the unit volume of liquid.

In the experiments with sulphuric acid, and the other acids above mentioned, the gas was conducted in a stream of small bubbles through a column of sulphuric acid before it was allowed to enter the apparatus.

The saline solutions employed were prepared by dissolving in pure water, deprived of air, by boiling as much of the salt as it would take up at the temperature of about 80°, and then suffering it to cool in a close vessel down to the standard temperature. The solutions were thus in contact with an excess of the solid, and could not fail to be saturated with the salt when used.

In many instances a slight rise of temperature was noted in the air of the flask early in the progress of the absorption; but the continued vibration of the flask in the midst of a large mass of water, kept at the standard temperature 60°, prevented this change from amounting, in any case, to more than two or three tenths of a degree, and had the effect of restoring the air of the flask to the standard temperature before the absorption was measured.

Of the Absorption by Sulphuric Acid.

The amount of CO_2 absorbed by this liquid, viz., ninety-four per cent. of the bulk of the sulphuric acid, was so unexpected a result, that on

our first trials we were unwilling to admit its correctness. But with chemically pure acid, numerous experiments, repeated on various occasions, have led to the same numerical determination. This is more than twice the amount absorbed in Saussure's experiment. But the difference is readily accounted for by the form of his apparatus almost precluding any agitation of the acid with the gas, while the true result can only be obtained by brisk and continued agitation in an atmosphere of the carbonic acid of unvarying tension. With our apparatus, we find it easy to complete the operation in twenty minutes, the results of the successive trials rarely differing as much as one-half per cent.

As, however, some doubt has been intimated by an English chemist as to this great absorbent power of sulphuric acid, we will here cite some experiments made with such simple means as are accessible in every laboratory, which, if repeated, will serve to demonstrate the point in question. In none of these experiments, however, is the full absorption reached, as, from the nature of the apparatus, this was impracticable.

1. One cubic inch of pure sulphuric acid, sp. gr. 1·838, was placed in a test-tube, and a stream of dry carbonic acid gas was passed through it for five minutes. On heating the liquid by plunging the tube in boiling water, bubbles of gas were evolved, but not very copiously; but the temperature being raised still higher by a lamp, a very brisk effervescence arose, and the gas continued to be evolved for several minutes, forming a deep foamy layer at the top of the liquid. This scientific experiment proves clearly enough that a larger volume of the gas is absorbed by the acid while allowed to bubble through it, and points to what takes place in the drying apartment of the instrument of Will and Fresenius.

2. One cubic inch of the same acid was introduced into a ground stopper bottle, and briskly agitated for a few minutes with the gas. It was then transferred to a graduated tube over mercury, the column of the latter standing ten inches above the level of the reservoir. This relief of tension caused at first a free escape of the CO_2 from the liquid. One cubic inch of previously boiled water was now let up, which, by raising the temperature, occasioned a still further evolution of gas. When this had ceased, a lamp-flame, applied to the upper part of the tube, liberated a large additional volume, and the liquid continued to yield bubbles of the gas even after being boiled for several minutes. When cooled at the temperature of the surrounding air, 75°, the instrument was transferred to a deep reservoir of water. The levels being adjusted, the

volume of gas was found to be 0·52 cubic inches. Of this, all but a very minute bubble was absorbed by agitation with the water.

Lest it should be supposed that in this, and the other experiments in which the acid liquid was boiled in contact with the column of mercury, some sulphurous acid might have been generated, we repeatedly performed the same experiment with boiled water and *unchanged* sulphuric acid, and although the liquid was kept for many minutes in active ebullition close to the surface of the mercury, the only gas evolved was a small bubble, and this was not readily absorbed by water.

3. One cubic inch of pure sulphuric acid was formed into a graduated tube. Carbonic acid was made to fill the rest of the space by displacement, and the whole was then quickly inverted over mercury. When the acid thus in contact with the gas was briskly agitated, the column quickly rose, and in a few minutes 0·5 cubic inches was absorbed. The experiment was discontinued when the absorption amounted to about 0·7 cubic inches.

4. A cubic inch of the same liquid was placed in a Liebig apparatus, and the whole counterpoised carbonic acid dried by transmission through SO_3, was then made to bubble for fifteen minutes through the bulbs. The gain of weight was found to be 0·36 grains. The sulphuric acid thus charged with the gas was transferred to a graduated tube over mercury, when the gas immediately began to escape. On adding a little water, and applying a brisk heat to the acid, a volume of gas was evolved amounting to 0·73 cubic inches, all of which, except a minute bubble, was re-absorbed by water. This falls a little short of the amount indicated by the gain of weight 0·36 grains, a result due to the imperfect separation effected in the last operation.

The force with which sulphuric acid retains the absorbed carbonic acid, even at an elevated temperature, is illustrated by the following experiment:—

One cubic inch of sulphuric acid, like the preceding, was saturated at 60° with the gas. The phial containing it was then exposed for fifteen minutes to the temperature of boiling water, by immersing it in a large capsule in which water was contained in active ebullition; minute bubbles of the gas were disengaged, but nothing approaching an effervescence took place. The loss of weight was 0·6 of a grain, which is less than 1-7th of the whole amount of gas united with the acid.

Error incident to the use of Sulphuric Acid, as an agent for drying Carbonic Acid Gas, when the quantity of the Gas is to be estimated.

The remarkable desiccating power of sulphuric acid has, as is wel

known, led to its employment in certain processes in which carbonic acid, and other gases, are to be deprived of their associated aqueous vapor before being measured or estimated by weight. In the apparatus for determining the amount of CO_2 in the atmosphere of hospitals or other places, as described by Orfila, the air is made to traverse tubes charged with SO_3 before passing into the Liebig bulbs, in which the CO_2 is absorbed. So in the researches of Boussingault and Leroy, on the proportion of CO_2 in the air of Paris and Andilly, those of Le Blanc on that of the air of confined places and mines, and those of Andral and Gavarret on the amount of this gas contained in the air expired from the lungs, sulphuric acid is used as a means of drying the air before exposing it to the potash solution.

In the ingenious instrument of Will and Fresenius, for determining the carbonic acid of alkaline or other carbonates, the gas is disengaged by the re-action of SO_3, and is compelled to traverse a considerable volume of this acid before making its escape into the air.

It is evident that, in all these arrangements, some carbonic acid must be retained by the liquid, yet it would seem that chemists have not contemplated such a result, for in the various published details of the processes here spoken of, no reference is made to this source of error, and the passage of the gas through the SO_3, without any absorption, appears to be assumed as an established fact.

To show that the amount of absorption in these cases may be too great to be neglected in *exact research*, we cite the following experiment:—

To a small two-necked bottle, charged with two cubic inches of SO_3, and counterpoised before the experiment, was adapted a long drying tube, filled with pure freshly prepared chloride of calcium. A capacious flask, charged with diluted hydrochloric acid, and furnished with a bent tube, was connected with the other end of the drying apparatus, the connections in both cases being made by gum elastic tube. A second tube was attached to the bottle by its other opening, for the escape of the gas. In terminating the experiment, an aspirating arrangement was adapted to this second tube. The CO_2 transmitted through the SO_3 in these experiments, was evolved from perfectly pure calc spar, introduced successively in small fragments into the flask containing hydrochloric acid.

To test the dryness of the CO_2, after transmission through the long drying-tube, we attached to the latter a short tube with chloride of calcium previously counterpoised. This acquired no sensible increase by

the passage of a large volume of the gas; we, therefore, conclude that no moisture could pass into the bottle containing the SO_3.

The amount of calc spar used was fifty grains, and in repeated trials we found the gain of weight of the SO_3 to vary but little from 0·63 of a grain. This corresponds to 1·43 grains carbonate of lime in the 50, and would involve an *error of between two and three per cent. in computing the quantity of carbonate from the amount of dry carbonic acid evolved.*

By a very prolonged aspiration of dry air through the SO_3, the greater part of the absorbed CO_2 is removed. But care must be taken at the same time to guard against the introduction of moisture with the air, by attaching an ample drying-tube, through which the air may enter the vessel. This last precaution ought never to be dispensed with in using the instrument of Will and Fresenius. We have found that aspirating *directly* into the flask from the external air, the apparatus ceases to lose weight, while a considerable portion of the CO_2 still remains in the SO_3, the addition of weight by moisture balancing the loss of the escaping gas.

In the drying apparatus of Orfila, and other similar arrangements for estimating the CO_2 of air, the volume of SO_3 used is probably not greatly less than in the above experiments. From its extensive surface in contact with the passing air, as well as the slowness with which the latter is transmitted, we may fairly infer the absorption of CO_2 to be larger.

If we assume the average amount of CO_2 in the atmosphere at $\frac{1}{3000}$, the whole of this gas contained in five cubic feet will be something less than three cubic inches. This volume of air passed through the drying tubes containing SO_3, would scarcely deposit less than one cubic inch, and thus the estimated amount of CO_2 present in the air would fall below the truth by one third of the whole quantity.

Nitric Acid.—In our experiment with this liquid, the absorption was very rapid, and terminated in about five minutes. By continuing the agitation, the movement of the column was reversed, and the flask was found to be filled with vapors of nitrous acid. On expelling the nitrous fumes by blowing into the flask, and then recharging with carbonic acid, a further expansion took place, accompanied by the escape of nitrous gas. The nitric acid employed, although not of the fuming kind, was mingled with some nitrous acid, which at a certain stage seems to have been displaced by the absorbed carbonic acid.

Hydrochloric Acid.—The first stage of these experiments presented

an anomalous effect. The gas in the flask was much expanded, causing a considerable depression of the mercury in the near limb of the measuring instrument. When at its maximum it amounted to twelve cubic inches, the regular absorption quickly followed, which was at first quite rapid, but soon became very slow, and ceased altogether in about ten minutes. This expansion is no doubt due to the sudden ascent of gaseous hydrochloric acid into the atmosphere of dry carbonic acid gas, in accordance with Dalton's law of gaseous diffusion. Something of the same kind, doubtless, occurred in the beginning of the experiment with nitric acid, but was not observed on account of the very rapid absorption of the gas by this acid, and the feeble diffusion of nitric acid vapor, compared with that of hydrochloric acid.

The rapid and large absorption of carbonic acid by hydrochloric acid, is well seen by inverting a tall glass filled with the latter in a capsule of the same, and placing a fragment of calcareous spar beneath. The bubbles, abundantly evolved below, will be greatly reduced, and but a small part of the disengaged gas will collect at the top.

Chloride of Calcium.—The solution of choloride of calcium was made by adding calcareous spar in small fragments to pure, somewhat dilute hydrochloric acid, previously boiled. The clear solution was decanted from the remaining spar, and evaporated until sufficiently concentrated to deposit some of the chloride as it cooled. At a temperature of 70°, it was transferred to the bottle in which it was to be kept during the experiments. When, by immersion in the main cistern, this vessel was brought to the standard temperature, a portion of the chloride collected in crystals at the bottom, and thus enabled us by agitation to insure the saturation of the liquid when used.

In nearly all the experiments with other liquids, the time required to terminate the absorption was so short, and therefore the change of temperature in the measuring tubes so small, as to make the correction due to this cause quite insignificant. Usually this change did not exceed $\frac{2}{10}$, and, in many instances, was only $\frac{1}{10}$ of a degree; and as it operated upon a volume of gas of from 1 to 1·5 cubic inches, it could not, even assuming the highest numbers here mentioned, be more than about $\frac{1}{1620}$th of a cubic inch, which is far within the limits of the errors of observation. In these cases the observed absorption is set down as the true. But in the experiments with the chloride of calcium, the entire absorption being very small, and the time occupied in agitation so great (35′) as to allow of a more considerable change of temperature in the measuring tube,

it became necessary to apply a correction for this change deduced from the observed temperatures and known dimensions of the instrument. In all such cases the number in the table has been corrected.

Chloride of Magnesium.—The solution was prepared by adding pure carbonate of magnesia to dilute hydrochloric acid, until, on heating the liquid, no effervescence was produced. It was then concentrated by gentle boiling, so that when cooled to about 70°, it deposited a part of the chloride. Lest in the process of evaporation any of the magnesia should have been deprived of its carbonic acid, a few drops of hydrochloric acid were added, on transferring the bottle from which it was to be used.

Phosphate of Soda.—The salt employed was such as we were using in the laboratory, and gave no distinct evidence of the presence either of an alkali or an alkaline carbonate. Using it in this state, we found the absorption to be enormous, and such as could only be explained by the presence of an alkali. Rejecting the first result, we rendered the saturated solution slightly acid by the addition of phosphoric acid, and in this state it gave the result recorded in the table.

Acetate of Soda.—The solution employed in the first instance was made from a very pure crystalline acetate, prepared by adding pure carbonate of soda to acetic acid. Finding the absorption very great, the solution was acidulated with acetic acid, and re-crystallized by gentle evaporation. A new solution was made with the product, and to this a drop or two of acetic acid was added, so as to impart to the liquid a decided acid reaction. With this saturated solution the result given in the table was obtained.

That none of this great absorption was due to the presence of alkali or carbonate in the liquid, was made apparent by the following experiments. The solution thus charged was withdrawn from the flask, and heated in a test tube. Violent effervescence occurred, until, by boiling for some time, all the free carbonic acid was liberated. On adding hydrochloric acid, and again heating, not the slightest disengagement of gas could be perceived. Supposing alkali to have been present in the solution, the carbonate formed could have been detected by the escape of gas; and had the solution originally contained a carbonate, the same result would have occurred. We are, therefore, satisfied that the above numbers represent the absorption by the solution of acetate.

Tartrate of Soda and Potassa.—Finding in our first trial with this salt a very extraordinary and continued absorption, we discontinued

the experiment, under the impression that the salt employed contained free alkali. We next repeated the trial with a similar solution, slightly acidulated by tartaric acid; but still found the same result. At first the absorption was very rapid; afterwards it became slower, but continued steadily to proceed. On examining the liquid in the flask, we found it to have become milky from a fine granular precipitate, which, after a time subsided to the bottom. To examine the effect more completely, we introduced some of the same solution into a small flask, and passed through for some time a stream of CO_2 from the pipe of the gasometer. The same milkiness gradually showed itself, and at length quite a copious precipitate of a white granular salt was collected at the bottom. Separating this by filtration, we found it to be but slightly soluble in water, and to have strong acid reaction. The clear liquid from which it had been separated was now evaporated to concentration. On the addition of hydrochloric acid, it effervesced rapidly. We infer, therefore, that in these experiments the CO_2 displaced a portion of the tartaric acid to form an alkaline carbonate, at the same time that the tartaric acid formed the comparatively insoluble super-tartrate which was deposited.

On comparing the absorbent power of the different saline solutions included in our table, it will be remarked that, in general, those which contain the largest amount of dissolved salt, absorb the gas in least quantity. A closer examination, however, will show that the absorption is not controlled by this condition alone. Substances of very different solubility give solutions of the same absorbent power. Thus the solutions of the nitrate of baryta and chlorate of potassa contain weights of these salts in the ratio of about 4 to 3, and are yet equally absorbent. Again, the solutions of nitrate of potassa and potash alum contain these salts in the ratio of 5 to 7, and yet the absorbing power of the former is 0·748, and that of the latter 0·763. These various forces of absorption cannot at present be reduced to any simple law; but they are, doubtless, dependent in part upon the chemical relations of the dissolved matter and gas, which are brought into mutual activity in absorption without being productive of any true chemical combination.

Professor Horsford inquired whether it was apprehended by the authors that the quantity of carbonic acid that would be retained by the liquids, in the process of Will and Fresenius would impair the accuracy of that process for technical purposes.

Dr. R. E. Rogers replied that while it might not be serious enough to be important in the examination of the commercial carbonates, yet

the error was quite too great to be admissible in delicate analytical processes, and especially in the determination of equivalents.

2. On a new Method of decomposing Silicates in the Process of Analysis. By Henry Wurtz, of New York.

Having had occasion in the course of some researches upon the green-sand of New Jersey, which will be presented to the Association hereafter, to observe the facility with which that substance is decomposed by a fusion with chloride of calcium, it occurred to me that this property of the earthy chlorides might be applicable in the analysis of minerals.

Some experiments were accordingly made with reference to this supposition. Felspar and hornblende were fused with chloride of calcium, and it was found that the masses thus formed could be entirely decomposed by hydrochloric acid. The use of chloride of calcium is, however, obviously attended with several inconveniences, such as its deliquescent properties, and the unavoidable introduction of a large quantity of ammoniacal salts into the solution, in the separation of the lime from it.

Chloride of barium was therefore substituted, and the results of experiments upon this re-agent were completely successful.

The chloride of barium of commerce often contains lead, resulting probably from the leaden vessels in which it is crystallized. It is necessary in this case to pass sulphuretted hydrogen through its solution. This solution is then filtered, recrystallized, and the washed crystals, dried on the sandbath and ignited, to drive off the water of crystallization. The pure chloride of barium thus obtained, is pulverized, and is then prepared to be used for the purpose here proposed.

Chloride of barium may be fused in a platinum crucible by a blast lamp, or by an alcohol blowpipe lamp. A mixture of chloride of barium and chloride of strontium in atomic proportions fuses, however, far more easily than either of its ingredients. Such a mixture is fused by the heat of an ordinary Berzelius lamp, and more easily, I think, than carbonate of soda. This is analogous to the well known fact, that a mixture of carbonate of soda with carbonate of potassa fuses more easily than either of the alkaline carbonates by itself. I have found, moreover, that although sulphate of strontia, when precipitated by itself, appears in a form somewhat gelatinous, tedious to wash, and difficult to filter, yet when precipitated in the presence of sulphate of baryta, it takes on the finely granular form of the latter, and the com-

bined sulphates are as easily washed as the sulphate of baryta when precipitated alone.

The atomic proportions of a mixture of chloride of barium and chloride of strontium, suited for the fluxion of silicates, are about four parts of the former to three of the latter.

The best mode of proceeding was found to be as follows:

The mineral in fine powder is intimately mixed with four or five times its weight of chloride of barium, or of the mixture of chloride of barium and chloride of strontium, in a platinum crucible, which is then covered, and exposed to a heat sufficient to fuse the mass for twenty or thirty minutes. When cool, the mass is loosened by bending the crucible, and allowed to fall into a beaker glass. Water is then poured on it in sufficient quantity to dissolve the excess of chloride of barium, or chloride of strontium, and the undissolved portion allowed to subside to the bottom of the beaker. The clear solution is then poured off into a porcelain dish, and concentrated hydrochloric acid is poured upon the residue in the beaker. The precaution of removing most of the earthy chlorides by means of water before adding the acid, is necessary, on account of the difficulty of decomposing the mass by the direct affusion of strong hydrochloric acid, owing to the insolubility of the chlorides of barium and strontium in this acid.

After the application of heat to the beaker until the residue contained in it is decomposed by the acid, its contents are also transferred to the porcelain dish, and the whole contents of the latter evaporated to dryness in the usual manner, for the separation of the silica. The filtrate from the silica is precipitated while hot with a slight excess of sulphuric acid, kept hot for half-an-hour, and then filtered. The filtrate from the sulphate of baryta must contain all the constituents of the mineral, except the silica, and these may be determined by the ordinary methods.

When the mineral thus treated contains sulphuric acid, it will evidently remain with the silica in the form of sulphate of baryta. A difficulty would also seem to occur when the mineral contains much lime, on account of the insolubility of its sulphate. This last difficulty is, however, obviated, in some degree, by the very considerable solubility of sulphate of lime in hydrochloric acid,—a fact which must have been noticed by many chemists.

The first qualitative experiment was made with a specimen of colorless transparent orthoclase from New York island. This felspar was fused with chloride of barium, according to the above process, and the

silica thus obtained was found, upon fusion with carbonate of soda, to be perfectly pure.

The next experiment was made upon a black crystallized hornblende from Franconia, New Hampshire. The silica obtained from this was found to contain considerably more than a trace of iron. I do not, therefore, venture to recommend this process at present for minerals which contain a very large quantity of oxide of iron. Many more experiments which time has not permitted me to make, will soon be made to settle this question, which I do not consider yet determined.

To test the chloride of barium process quantitatively the mineral called pink scapolite, of Bolton, Mass., long ago analyzed by Dr. Jackson, was selected. My results agree entirely with his, except as regards the presence of lithia and oxide of cerium, which careful qualitative examination did not enable me to detect.

This mineral was found, contrary to recorded statements, to be not completely decomposed by concentrated acids, even when finely elutriated. A determination of the silica, made by decomposition with hydrochloric acid, gave the per centage 50·25; another, made by fusion with carbonate of soda, gave 47·55.

Two determinations of the specific gravity, made upon the identical portion of mineral which was analyzed, dried at 212° in coarse powder, gave the numbers of 2·7002 and 2·7046.

The results of a fusion with chloride of barium were as follows:—

		Oxygen.	
Silica	47·69	2·478	24·78
Alumina	25·75	12·035	12·71
Peroxide of Iron	2·26	·68	
Lime	17·31	4·82	7·08
Soda	7·76	2·26	
Protoxide of Manganese, trace			
	100·77		

Agreeing tolerably well with the received formula of scapolite

$$(\dot{C}a\,\dot{N}a)^3\,\dddot{S}i^2 + 2\,\dddot{A}l\dddot{S}i.,$$

which requires for the oxygen of the silica peroxyds and protoxyds the ratio 4 : 2 : 1, while the above analysis gives the ratio 4 : 2·06 : 1·14.

This analysis was, however, through lack of time, made hastily, and will be repeated at the first opportunity.

This method appears to possess advantages, for decomposing silicates which contain both the alkalies, over the ordinary methods of fusion with the hydrate or with the carbonate of baryta. Hydrate of baryta

generally acts upon the crucible, causing the mass to adhere to it, and upon the affusion of hydrochloric acid, any potash which the mineral may contain, consequently enters into combination as bichloride of platinum and potassium, and remains with the silica. The carbonate of baryta process requires an intense heat, and is difficult of execution.

The chloride of barium process proposed in this paper is probably not more laborious than an ordinary carbonate of soda fusion, and is applicable in cases in which the silicate contains both potash and soda.

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3. On Canadian Localities of Ilmenite and Chromic Iron, with Remarks upon their Associations with the Gold of Canada and California. By T. S. Hunt.

[*Not received.*]

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4. An Account of several new Mineral Species. By Prof. C. U. Shepard.

1. *Dysyntribite.*

Massive; composition granular, mostly impalpable.

Fracture even, splinters, but hard to pulverize.

Surface almost dull, very little glistening.

Lustre resinous, feeble; color dark green, grayish or yellowish, sometimes mottled with red and black.

Hardness = 3.5 . . . 4.0. Sp. Gr. = 2.76 . . . 2.81.

Before blowpipe in an open tube, emits moisture and turns whitish. Alone before blowpipe in thin fragments, fuses into a white, porcelainous glass. With borax, dissolves slowly into a transparent glass. It is partially attacked by long boiling in sulphuric acid. It is composed of

		Oxygen.	Ratio.
Silica, - - - -	47·68	24·77	20
Alumina, - - - -	41·50	19·39	16
Protoxide iron, - -	5·48	1·21	1
Water, - - - -	4·83	4·29	3
Lime and magnesia in traces,			
	99.49		

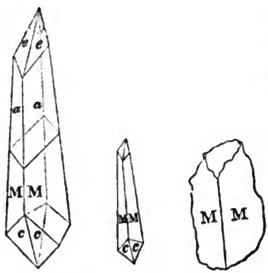
which leads to the following formula:

$$16\dddot{Al}\ \dddot{Si} + \dot{Fe}^3\ \dddot{Si}^4 + 9\dot{H}.$$

This mineral is very abundant in St. Lawrence Co., N.Y., and probably in several of the contiguous counties. It occurs in considerable masses in immediate connection with the specular iron of that region. It has hitherto passed under the name of Serpentine. The specimens from which the foregoing description has been derived, came mostly from the town of Rossie. A variety from Natural Bridge in Diana was found to possess a very remarkable degree of toughness, to be more fusible, and to contain a decided proportion of phosphate of lime. This species appears to exist at Monroe, Orange Co., N.Y., associated with the magnetic iron ore beds of that place. The color, however, is much darker; and it has hitherto passed under the name of indurated Talc.

The name Dysyntribite is derived from the words Δῦς, (*hard,*) and Συντρίβω, (to *crush,*) in allusion to the difficulty experienced in crushing the mineral beneath the pestle to a fine powder.

2. *Rutherfordite.*



Primary form, oblique rhombic prism M on M, about 93°

The figures in the margin represent three crystals with nearly the dimensions of the drawings themselves. Their faces are all too dull and uneven for determining the value of their inclinations. No cleavage.

Fracture conchoidal, lustre of fracture shining and resinous. Color, yellowish brown, opaque.

Hardness = 6.5. Sp. Gr. = 5.58 . . . 5.69.

When heated in a glass tube by means of blowpipe, it cracks to pieces, glows as if on fire, emits much moisture, and turns yellow. By itself, it is infusible; but with borax, dissolves slowly into a clear yellow glass.

It is decomposed with difficulty by long boiling in hydrochloric acid, but is easily attacked by sulphuric acid, the solution at first formed subsequently letting fall an abundant white powder. A portion of the solution after being filtered was boiled, which induced immediate milkiness, thus evincing that the metallic acid present was probably the titanic.

A portion of the sulphuric solution was decomposed by ammonia, without indicating the presence of iron or manganese. Hydrochloric acid was boiled upon the precipitate, and the solution obtained on being treated with concentrated sulphate of potassa; a very copious granular white precipitate was the result. This was separated, dissolved in hot water, decomposed by ammonia, and the precipitate ignited. Its color was at first pale yellow; but on subsequent heating, with exposure to the air, it passed to orange yellow, which led to the inference that it was oxide of cerium (possibly including traces of oxide of uranium). Yttria may also be a constituent of the mineral, which besides was found to contain traces of lime.

It occurs in crystals and grains at the gold mines of Rutherford county, North Carolina, where it is associated with Samarskite, Rutile, Brookite, Monazite, Zircon, &c. Its name is bestowed in allusion to its locality.

3. *Paracolumbite.*

Massive; diffused in grains, and short, irregular seams.

Color, iron-black, sometimes with a faint tinge of purple. Lustre imperfectly metallic.

Fracture sub-conchoidal, opaque, streak black.

Hardness about 5·0.

In an open tube, decrepitates slightly and evolves a little moisture, which possesses a feebly acid reaction; but the heated mineral does not change color. Heated in platinum forceps, or on charcoal, it fuses readily into a black glass, which is not magnetic. With borax, it dissolves rapidly, and yields a glass which is yellowish-brown while hot, but becomes paler on cooling. When decomposed by sulphuric acid in a glass tube, the glass is corroded by hydrofluoric acid. The mineral is easily attacked by sulphuric acid, and a heavy white precipitate (unmixed with silica) is obtained. A portion of its filtered solution was not rendered milky by boiling; nor did the white precipitate evince a tendency to traverse the filter, on the affusion of hot water. A portion of the filtered solution was decomposed by ammonia, and the precipitate of mixed oxides of iron and uranium was treated with carbonate of ammonia, which dissolved the latter, and showed its proportion to be small, when compared with the former. From these experiments the mineral seems to be composed of the oxides of iron and uranium in combination with a metallic acid, which is not the titanic.

This mineral occurs about one mile in a south-westerly direction from the village of Taunton, in Massachusetts; disseminated in exceedingly

minute quantities through a very large boulder of granite, lying in a field contiguous to the highway. The granite is almost wholly made up of a peculiar greenish-white feldspar. My attention was first directed o the mineral by Dr. Hitchcock, at the time he was engaged in he geological survey of the State.

It is named from its resemblance to columbite.

4. *Houghite.*

Massive; in oblong, flattened, reniform concretions, rarely above $\frac{3}{4}$ths of an inch long, with botryoidal surfaces. Externally, and to the depth of $\frac{1}{16}$th of an inch, of a milk-white color: within, bluish or reddish-white. Structure, curved lamellar, (sub-fibrous,) nearly compact. Rather difficult of fracture. Opaque; lustre faintly pearly, glimmering. The concretions often embrace minute crystals of pale red spinel: sometimes a single large and perfect octahedron of this mineral forms nucleus for the Houghite.

Hardness = 2·5. Sp. Gr. = 2·02 . . . 2·03.

On being heated before the blowpipe, it decrepitates. In a glass tube, it separates into several pieces, and emits abundance of water. Alone on charcoal, or in platinum forceps, it phosphoresces slightly, becomes pearly white, but does not melt. With borax, it dissolves into a transparent glass, slightly yellowish while hot. The mineral does not give the test of phosphoric acid, with boric acid and the iron wire. Solution of cobalt gives a deep blue color, after long heating.

It is almost wholly soluble (before and after ignition) in the strong acids, unattended with the separation of silica. Ammonia throws down a bulky white precipitate from the solution. The cleared liquid then fails to give a precipitate with oxalate of ammonia, but an abundant one with phosphate of soda. The bulky precipitate obtained in the first instance is about half taken up by potassa, from which the hydrochlorate of ammonia precipitates alumina. What may be the composition of the residue not attacked by the potassa, I have not determined. It appears, however, to contain only a trace of iron, and to be wholly destitute of silica. The amount lost 33·33 p. c. by ignition. It, therefore, in the present stage of the investigation, appears to be a hydrate of alumina and magnesia, differing from Gibbsite not merely in the content of magnesia, but physically by possessing a lower hardness and specific gravity. It also approaches Völknerite, but differs from it in containing much less both of water and of magnesia. Its true chemical

character seems to be that suggested above, and which its mineralogical association still further renders probable, viz., as follows, Mg $\ddot{A}l$ (spinel) with water.

I met with this mineral about ten years ago, while on a mineralogical journey with the late Baron Von Lederer, at a locality of yellowish serpentine, situated about two miles from the village of Oxbow, in St. Lawrence county, N. Y. But my attention has more recently been directed to the subject by much better specimens from the neighboring town of Rossie, which have been sent to me by Dr. Franklin B. Hough, of Somerville, in the same county, in compliment of whom I have given it the above name.

5. *Marasmolite.*

Primary form, cube; secondary, a combination of the octahedron, with the rhombic dodecahedron. Faces, drusy. Cleavage, cubical, not distinct. Fracture uneven.

Lustre, adamantine. Color, brownish black. Streak, reddish brown. Opaque. Brittle. Hardness = 3·5. Sp. Gr. = 3·73 . . . 3·74.

When heated before the blowpipe, it decrepitates violently, unless heated very slowly, when a portion remains on the charcoal, turns black, fuses partially with ebullition, covers the coal with a yellow powder, and becomes magnetic; but the assay remains of black color after cooling, which is unlike blende under the same circumstances. When heated in an open tube, it emits sulphur fumes, and the tube is lined with a yellow deposit, which becomes white on cooling. With hydrochloric acid it emits sulphuretted hydrogen. It dissolves rapidly in nitric acid, with separation of sulphur.

It consists of

		Atoms.	Ratio.
Sulphur, -	38·65	19·	5
Zinc, - -	49·19	12·	3
Iron, - -	12·16	3·55	1
	100·02		

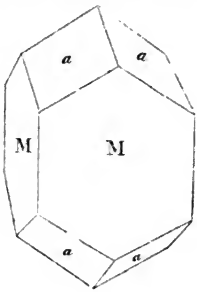
$$3\,ZnS + FeS^2.$$

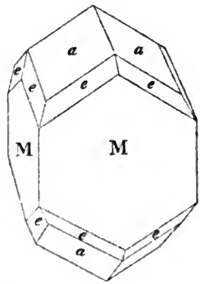
This mineral was found by me when engaged in the mineralogical survey of Connecticut, at the "China-stone quarry," near Middletown, where it occurs in small quantity, imbedded in felspar, along with columbite, pitchblende, and various other interesting minerals.

Its name is bestowed in allusion to its proneness to disintegration, from Μάρασμὸς, *decaying*.

6. *Calyptolite.*

Primary form, right square prism.
Secondary forms, as in the figures.





a on *a* from 122 to 124°.

Surface M not smooth, often somewhat concave; *a* more even, and feebly shining. Lustre adamantine. Color dark brown. Streak, dark ash-gray. Opaque.

Hardness = 6·5. Sp. Gr. = 4·34.

Heated in an open tube, it yields moisture, which exhibits an acid reaction, and the mineral turns to a pale yellowish white. Alone, before the blowpipe, it turns nearly white, but does not fuse. It does not fuse readily with soda; with borax dissolves slowly into a glass which is yellow while hot, but colorless on cooling. When heated in powder with sulphuric acid for some time, it appears to be completely decomposed, and the glass tube exhibits corrosion.

The quantity of the mineral was too small for a satisfactory examination; but the absence of silica, lime, magnesia, and alumina was ascertained; and the probability that the substance is a fluo-columbate of some of the less common earths and oxides, established.

The mineral was first noticed by me upwards of twenty years since, at the chrysoberyl locality of Haddam, Conn., and was then mistaken for zircon, under which name it has, I believe, been generally known in cabinets to the present time. It occurs in exceedingly minute crystals,

diffused through albite, garnet, beryl and chrysoberyl. I possess the same substance also from the China-stone quarry in the neighboring town of Middletown.

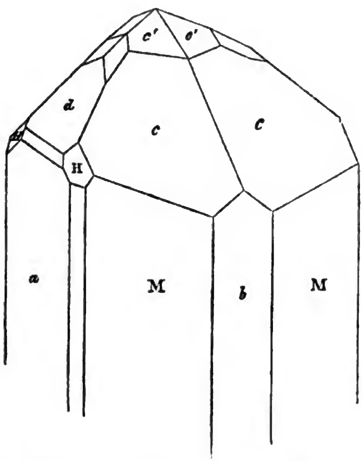
It is named from Καλυπτος (*covered*), in allusion to its long concealment from recognition by mineralogists.

7. *Eumanite.*

Primary form, right rhombic prism. M on M = 123°.

Secondary form, M on M = 123° 0′.

"	*a*	118° 0′ to 118° 30′.
"	*c*	136° 0′.
c	"	151° 30′.
"	*c′*	159° 30′.
"	*d*	127° 30′.
a	*d*	127° 40′.
H	"	144° 20′.
a	H	128° 20′.
c	"	156° 30′.



Surface of M. rather imperfect, and not possessed of a high lustre; the other faces very brilliant. Color blackish brown, (resembling cer-

tain crystals of tin ore.) Translucent. Color, deep red, (like almandine garnet.)

Hardness above 6·0.

But a single crystal, and that less than a grain in weight, has thus far been observed. It was attached with albite, and occurred in the tourmaline vein in Chesterfield, Mass. The specimen has been in my possession for many years, and the description of it deferred in the hope of obtaining additional specimens of the mineral, which has thus far been disappointed, with the exception of a mere speck in the massive state, and which is also attached to albite.

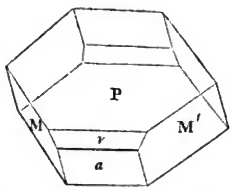
The species is named from Eυ and Μανὸς (*rare*,) from the supposed scarceness of the substance.

8. *Corundophilite.*

Primary form, oblique rhombic prism. M on M about 120°, P on M′ 97° 30′.

Secondary prism, M on M′ about 60° 0′.

P	"	"	97° 30′.
"	*a*	"	88° to 89°.
"	*v*	"	122° 30′.



Cleavage parallel with P, highly perfect: also traces parallel with M, and the shorter diagonal of P.

Lustre, common pearly, upon perfect cleavage faces.

Color, dark leek green, passing into gray and greenish black. Streak corresponding to color. Translucent.

Sectile. Thin laminæ flexible, but less so than talc.

Hardness = 1·5 . . . 2·0. Sp. Gr. = 2·862.

Compound varieties.—The imperfectly stellular groups, columnar, implanted on corundum; also, spreading out into flat individuals which are interlaminated with corundum and emerylite.

It affords a little moisture when heated in a glass tube. Alone, before the blowpipe, it instantly turns black, and melts, without phosphorescence, at the extremity, into a shining black globule. It dissolves rapidly in borax with effervescence, into a clean bottle green glass. In analyzing 0·146 gramme of the mineral, I obtained in the 100 parts, of silica 34.75, of protoxide of iron 31·25, of alumina 8·55, and of water 5·47. Thus making the loss nearly twenty per cent., a portion of which I am inclined to attribute to the alkalies. Neither lime nor magnesia was detected in the mineral.

My notice was first called to this substance as occurring in the large lump of blue corundum, (25 lbs. weight,) found by Hon. Mr. Clingman near Asheville, Buncombe county, North Carolina. The crystal above figured, though very perfect, was exceedingly minute, (less than $\frac{1}{5}$th of a grain in weight,) and was completely imbedded in a crystal of emerylite. Mr. Nuttall has called my attention to the mineral here described, as of frequent occurrence along with the carnatic corundum. He had noticed it as a peculiar variety of mica.

It is named from the words corundum and φιλὸς (*a friend*), because it appears to be an associate of that mineral.

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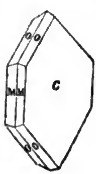
5. On new localities of American Minerals. By Charles Upham Shephard, M. D.

1. *Diaspore at Trumbull, Conn.*

This mineral occurs in very limited quantity in the well known fluor and topaz vein, which traverses an extensive bed of primitive limestone. It presents itself in small transparent and very brilliant crystals, as well as in closely aggregated, somewhat radiating folia. They are implanted upon fluor and margarodite, the more perfect crystals being attached to the latter mineral where it affords cavities.

The crystals are attached by the planes M M, and never exhibit more than half the form delineated in the annexed figure, viz., that portion which is to the right or left of a vertical diagonal of plane *c*.



M	on	M	—	130° 30′
"	—	*c*	—	114
"	—	*o*	—	125
o	—	*o*	—	152 30
o	—	*c*	—	104 30

The facs M and *o* are highly perfect; *c* is rather wavy. Cleavage perfect, parallel with *c*.

Hardness = 7·00 . . . 7·50. Sp. Gr. = 3·29.

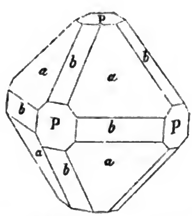
Before a blow-pipe, it instantly loses its transparency, turns milk white, phosphoresces strongly, and on its thinnest edges becomes rounded, in the most intense heat of the instrument. It affords abundance of water, when heated in the open tube. With borax it slowly melts into a colorless glass. It afforded by analysis

Alumina,	-	-	84·9
Water,	-	-	15·1
			100·0

This is the mineral announced by me many years ago as Euclase, which species (aside from its discrepancy of composition) it more closely resembled than any descriptions of Diaspore then existing in mineralogical books. The hardness now established for this species will henceforth remove it from the natural history order of *Spar* to that of *Gem*, a position which its high lustre, transparency, and property of dichroism still further entitled it to maintain.

2. *Ores of Uranium, at Middletown, Conn.*

These occur at the China-stone quarry, about two miles from Middletown. The *Pitchblende* is in small but very perfect crystals, having the form of the figure in the margin, which, so far as the faces *b* are concerned, has never before been observed. The crystals are imbedded in felspar.



Along with the uranochre of this locality, I find traces of the *carbonate of uranium*, and likewise of *sulphate* (the *Johannite*).

3. *Ores of Bismuth, at Haddam, Conn.*

The Bismuthene was noticed by me at the chrysoberyl repository

many years ago. I have now to communicate the occurrence of the Bismuthite and the Bismuth-ochre at the same place. The former exists in thin coatings upon crystals of Bismuthene; while the latter is found in a pulverulent form disseminated among various minerals, and is usually of a yellow color.

4. *Samarskite* (*Uranotantalite*); *in Rutherford County, N. C.*

This mineral was sent to me for examination by Hon. Mr. Clingman, of Ashville, N. C. It was in the form of angular grains (the fragments of large crystals), some of which weighed quarter of an ounce. They had been collected from the washings of some one or more gold mines, and were accompanied by Rutile, Rutherfordite, (nov. sp.) Corundum, Monazite, Brookite, Zircon, and several other more common species.

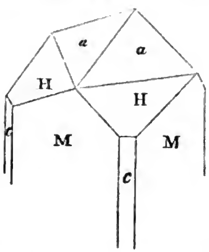
The more crystalline fragments evinced that the form was analogous to that of Columbite. Color velvet-black. Streak dark reddish-brown.

Opaque, H. = 5·5. Gr. = 5·69.

When first heated in a glass tube, it decrepitates, flies into pieces, glows slightly, after the manner of Gaudolinite; but remains of a black color.

5. *Thorite; at Danbury, Conn.*

The mineral here described is either this very rare substance, or it is a new species. Never having seen a specimen of Thorite, and no account of its crystalline form having been published by Berzelius (the author of the species), I am forced to rely upon less important traits for establishing the supposed identity, than could perhaps be desired. Primary form, right square prism. Secondary form, see figure.



M	on	M	—	90°
"	—	*c*	—	135
"	—	*a*	—	120
a	—	*a*	over summit	98
a	—	H	—	160

All the planes of the crystal (which is only $\frac{1}{20}$ of an inch long) are

bright and shining: H is less distinct, and present only on one face.

Cleavage imperfect. Fracture uneven to conchoidal. Lustre resinous. Color black. Opaque. Hardness, 5·0 to 6·0. Heated before blow-pipe in an open tube, it decrepitates slightly, emits abundance of moisture, which possesses a decidedly acid re-action. The mineral turns light brownish red, and on being heated in the platina forceps, does not suffer fusion, but presents at the point where it is most intensely heated a semi-fused or lava-like aspect. With borax, it easily fuses to a glass colored by iron. It occurs in very small crystals disseminated through feldspar (necronite) at the Danburite locality.*

6. *Sulphato-carbonate of Copper, at Bristol, Conn.*

A mineral having this composition occurs at the Bristol copper mine, in the form of a dull verdigris green coating upon crystals of vitreous copper ore. It is partially soluble in water; but the addition of acetic acid takes it freely into solution with effervescence, the solution yielding a precipitate with chloride of barium. Should this mineral when more fully examined prove a definite chemical compound, as I now suspect, it will then be proper to bestow upon it a mineralogical name.

Remarks on Ozarkite.—This species, described by me as occurring along with Schorlomite and Elæolite at Magnet Cove, Hot Springs Co. Ark., has been referred to Scolezite by Whitney, and is said to be mixed with apatite by Dana, (see his Mineralogy p. 302.) I have therefore been led to investigate the substance anew, so far as a very limited supply of material would permit.

Its sp. gr. = 2·24, which certainly brings it near to Mesotype; but it differs physically from that species, in constantly exhibiting a massive structure, its composition being small grained, angular, and passing thence to compact, resembling Petalite. Its hardness, = 4·5. Before the blow-pipe, it melts easily into a colorless glass, without swelling up like a worm, (whence Scolezite derives its name) and also without phosphorescence. Still further, it differs from Scolezite in not gelatinizing

* There is associated with the feldspar and Danburite a singular variety of altered pyroxene. It is in crystals and massive, of a greenish white color and vitreous lustre. Gr. = 3.117 to 3·125. Hardness, = 5·0 to 5·5. It fuses with ebullition into a colorless and semi-transparent glass. It is decomposed by sulphuric acid, with formation of sulphate of lime and magnesia, and separation of phosphoric and traces of boric acid. *October,* 1850.

with acids: it is simply decomposed by them, with separation of silica. I obtained on little more than half a gramme,

Silica,	—	40·91
Alumina,	—	15·75
Phos. lime	—	4.17
Lime,	—	4·52
Water,	—	15·10

and a large but undetermined quantity of soda and potassa. In this result, I regard the phos. lime as accidental, and the 19·55 p. c. of loss is probably too large for the alkaline content of the mineral. Accordingly, Ozarkite very illy agrees with any result ever obtained for Scolezite or Mesotype, of which mineral Scolezite is now regarded as a variety.

Along with the descriptions of characters, composition, and localities, a number of specimens themselves were exhibited to inspection.

Mr. George J. Brush remarked, that as his own name had been mentioned by Mr. Shepard, in connection with Ozarkite, he would state that in his examination he had obtained in it 77·41 per cent. of phosphate of lime, and that the specimen was apparently a mixture of apatite and a zeolite, which was sustained by qualitative analysis; what zeolite was not ascertained, as, with the utmost care in separation, it was found impossible to obtain any of the mineral which would not give the reaction of phosphoric acid.

6. Description and Analysis of Allanite from Franklin, New Jersey. By Dr. C. T. Jackson.

This mineral was discovered by Mr. Alger and myself last month. On first view the crystals reminded us of Columbite, which they resemble in appearance. By means of the blowpipe I ascertained that the mineral was Allanite, and subsequently, by chemical analysis, I proved it to be that mineral. The analysis, so far as I have completed it. gives for the composition of this mineral—

Water,
Silica,
Alumina,
Oxide of cerium,
Per oxide of iron.

The silica amounted to 33 per cent.
Oxide of cerium to 18 "

The other ingredients are not yet quantitatively determined.

This locality will furnish mineralogists with an abundance of specimens, the heaps of feldspar or labradorite, at the Old Iron Mine, near the house of the post-master of Franklin, Mr. Brown, being filled with them. It is an interesting mineral, as containing the rare metal cerium.

Specimens were exhibited to the section.

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7. Description of Bismuthic Tellurium or Tetradymite, from the Gold Mine of Whitehall, Virginia, with an Analysis of the Mineral, and its Relations to the Gold associated with it. By Dr. C. T. Jackson.

Some years since I announced in the pages of the *American Journal of Science*, that I had discovered Tellurium, associated with the gold of Virginia. At the time I possessed but little of the mineral, and could therefore give but a blowpipe assay of the ore. I therefore overlooked the existence of bismuth as one of its constituents, and mistook it for lead from the appearance of the oxide in the cupel. Mr. Fisher, subsequent to my publication, obtained some specimens of the ore from the United States Mint at Philadelphia, and made a chemical examination of it. He discovered the bismuth, and he also supposed he had proved the mineral to be a seleniuret and telluret of bismuth.

On visiting the mine, I obtained a sufficient quantity of the ore for chemical analysis, and made two analyses of it.

I give here the most correct of those analyses:

Tellurium,	35·05
Bismuth,	58·80
Sulphur,	3·65
Peroxide iron, gold and silex, (gangue and mixture,)	2·70
	100·20

A mere trace of selenium exists in this ore. The sulphur I determined by separating it, not only in the state of sulphate of baryta, but also in the state of yellow globules of pure sulphur, so there cannot be a doubt as to the occurrence of sulphur instead of selenium as an essential constituent of the ore. The gold exists between the folia of the tetra dymite, and is not combined. It is so universal in my specimens as to-

present the accurate determination of the specific gravity of the mineral.

Specimens were exhibited to the Section.

The Section adjourned, to meet at 3 P. M.

FIFTH DAY, FRIDAY, AUGUST 23, 1850.

SECTION OF CHEMISTRY AND MINERALOGY.

(*Afternoon Session.*)

The Section met at 3 o'clock, P. M., in its usual place. In the absence of the chairman of the day, (Prof. W. R. Johnson,) Dr. C. T. Jackson was called to the chair; Dr. R. E. Rogers, Secretary. At the request of the Secretary, Mr. Henry Wurtz, of New York, was appointed Assistant Secretary.

The following communications were made:

1. On the Availability of the Greensand of New Jersey, as a Source of Potash and its Compounds. By Henry Wurtz, of New York.

The vast importance of potash and its compounds in the arts, long ago impressed upon chemists the necessity of finding some source of these compounds other than the ashes of the forests. Accordingly, a great multitude of experiments have been made upon feldspar by various chemists, in the hope of attaining this important end; but owing to the peculiar chemical and physical properties of feldspar, the success of these researches has been doubtful.

It is true that the feeble affinities exerted by mineral waters impregnated with carbonic acid, and even those of pure water itself, acting through hundreds of years, have been found sufficient to break down the constitution of this hard silicate, thus giving rise to vast beds of kaolin, but when it is attempted to effect the same thing within any reasonable time, by artificial means, it is found necessary to bring into play temperatures and forces too enormous in amount to be economical in practice.

I have not been able to ascertain that any one has yet proposed to use, as a means of obtaining the end under consideration, the vast deposits of the potash mineral called *Greensand*, which exist in the United States; and it is the object of the present paper to show that this substance is far superior to feldspar in its adaptation to this purpose.

The existence and common attributes of the greensand are generally known; but a few words on this subject will not be amiss. It exists in greater or less quantity in several States, but has its greatest development, I believe, in the State of New Jersey, where it forms a stratum of variable thickness, covering a great portion of the counties of Monmouth, Burlington, Gloucester, and Salem. Wherever it occurs it is spread upon the land in large quantities as a fertilizer, and is a source of wealth to the farmers, to whom it is known by the name of *Marl*. Its properties as a fertilizer are undoubtedly owing to the ease with which the potash which it contains is abstracted from it by atmospheric agencies, as is suggested by Prof. Henry D. Rogers, in his report upon the geology of New Jersey, in which he has devoted about a hundred pages to the greensand, and has given many analyses which indicate the presence of from ten to thirteen per cent. of potash. No analyst has, to my knowledge, found potash in the English greensand, the fertilizing properties of which appear to be due to the presence of phosphates.

The external appearances of the New Jersey *marl* are very various. Some varieties are almost entirely composed of grains of greensand; others contain variable proportions of a red or brown earth and of quartz. A few contain more or less carbonate of lime, in such a form that it is not acted upon by dilute acids in the cold, although upon the application of heat a violent effervescence appears. Many contain iron pyrites, and some a trace of sulphate of iron. No variety which I have examined has yielded any phosphoric acid.

The greensand grains themselves contain, besides potash, silica, alumina, one or the other or both of the oxides of iron, and water, with sometimes a little magnesia.

The invariable development of a smell of formic acid by the action of strong sulphuric acid upon them, seems to indicate the presence of a little organic matter. Analyses of two varieties of the marl from the estate of the late Alfred Bishop, of Bridgeport, at Shrewsbury, Monmouth county, yielded the following results:

	I.	II.
Silica, ...	48·24	47·83
Alumina and oxide of iron, principally peroxide,	32·89	34·98
Potash, ...	6·38	4·94
Magnesia, ...	2·60	
Hygroscopic water, ...	4·81	11·50
Combined water, ...	5·69	
	100·61	99·25

It may be remarked here that Prof. Rogers' results were obtained upon the greensand grains, separated as much as possible from intermixed earth and sand, while the above were made upon the impure marl itself.

For more detailed information, I must refer to the elaborate report of Prof. Rogers, before mentioned. I will only state farther, that the greensand grains are easily pulverized, having only about the hardness of gypsum, and that they are decomposed by dilute acids, and we then come to the immediate subject of this paper.

Considering that the greensand contains the constituents of *alum*, with the exception of the sulphuric acid, it seemed probable that by the action of the sulphuric acid upon it a solution would be formed containing more or less alum. Many experiments were made, and much time was spent upon this point, without much success. It seemed as if the protoxide of iron was dissolved by the acid much more readily than any other constituent of the marl, and also that the organic matter interfered in some way. The solutions obtained had generally a dark brown color, and a smell resembling that of formic acid. They contained much sulphate of protoxide of iron, and gave a few impure crystals of alum.

A portion of the greensand was now gently ignited, which served the purpose of destroying the organic matter, if any was present, and also of peroxydizing the iron, thus rendering it less soluble in acids. The pulverized and ignited marl presented the appearance of a brownish red powder. It was easily decomposed by dilute sulphuric acid, yielding a solution, the contents of which, upon analysis, proved to be principally common alum, together with small quantities of iron-alum, and of the persulphates of alumina and iron. The first crystals of alum, obtained from a considerable mass of the solution, were almost perfectly pure, and upon the addition of a small quantity of chloride of

potassium to the solution, it was found, as might have been predicted, that all the iron was converted into the uncrystallizable perchloride, the sulphate of potash, thus formed by double decomposition, combining with the free sulphate of alumina to form common alum; and even in the last crops of crystals now obtained, little or no iron could be detected. The manufacture of alum, therefore, by the action of sulphuric acid upon previously ignited greensand marl promises to be successful beyond all anticipation. It is obvious that it will be necessary to select varieties of the marl as free as possble from lime and magnesia, which would cause a waste of acid.

My researches were next directed towards the production of chloride of potassium. Attempts to form this substance by the direct action of hydrochloric acid upon the ignited marl were unsuccessful. A very large quantity of perchloride of iron was formed, which would give rise to too great a loss of acid.

It was next found that by fusing together greensand and chloride of sodium at a red heat, a hard mass was formed, which yielded with water a solution containing potash; but owing to the difficulty of separating chloride of potassium from chloride of sodium when the latter is present in greater quantity than the former, this observation was considered as of little value.

A widely different conclusion was arrived at when chloride of calcium was substituted for chloride of sodium. The pulverized and ignited marl was mixed with a sufficient quantity of chloride of calcium to form upon the fusion of the latter a pasty mass. The decomposition of the greensand takes place in this case at a low temperature, and is so complete, that I have founded upon this circumstance a method of decomposing minerals in the process of analysis, which I have had the honor of presenting to the Association before.

It is evident that the combined water of the greensand must be expelled by ignition previous to fusing it with chloride of calcium, otherwise a quantity of the fused chloride of calcium will inevitably be decomposed by the steam evolved. The fusion may also be performed in close vessels, to avoid the decomposition which chloride of calcium undergoes when fused, in contact with the air. The mass, after fusion, falls to pieces in water, yielding to this solvent, in most cases, all the potash which was contained in the greensand employed in the form of chloride of potassium. The separation of this from the excess of chloride of calcium is an easy problem, owing to the difference between their solubilities.

This application of chloride of calcium will open a market for the large quantities of the substance which are thrown away in some manufactories of soda-ash.

All attempts to procure sulphate of potash by the fusion of various sulphates with the greensand were unsuccessful. In fact, the greensand itself, at a temperature below the fusing points of the sulphates of lime and magnesia, fuses to a black glass, which is no longer decomposable by acids.

A great number of other experiments were made upon the greensand, but no results were arrived at which promise to be of any practical value, except the above.

A very great number of experiments were also made, having for their object to obtain sulphate of potash by fusing together chloride of potassium with alum and with various sulphates, such as those of iron, magnesia, and zinc, which gave results of great practical value; but as these results had but a fortuitous connection with those upon the greensand, I shall not introduce an account of them here.

I will merely remark, that if sulphate of potash can be obtained by fusing together alum and chloride of potassium, both of these being obtained economically from the greensand by the above process, it is evident that this sulphate of potash may be treated in the same manner for the production of potash as sulphate of soda is in the manufacture of soda-ash; and it seems to me that the desideratum of another source of potash is thus supplied.

Dr. R. E. Rogers questioned whether there would not be too serious a loss of acid occasioned by the iron, alumina, lime and magnesia, in the greensand, to admit of its being an economical substance for alum, but concurred with Mr. W. in thinking that this could only be determined by experimenting on the large scale.

A further discussion ensued respecting the nature and origin of the greensand, in which Professor Shepard, Mr. Wurtz, and Dr. R. E. Rogers participated.

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2. A Notice of Foreign Meteorites. By Prof. C. U. Shepard.

1. *Tuttehpore, Hindostan, Nov.* 30, 1822.

This stone, so far as I am informed, has not been described. It is barely mentioned by Prof. Partsch, in the Appendix, p. 142, of his

Catalogue of Meteorites in the Imperial Collection at Vienna (1843), as not yet brought into Europe. While in Edinburgh last year, I was informed by Alexander Rose, Esq., that a fine specimen of this locality existed in the cabinet of Thomas McPherson Grant, Esq., by whom I was very obligingly presented with a fragment, and the means of making the present communication.

The fall took place in the evening at Tuttehpore, which is situated seventy-two miles from Allababad, on the Cawnpore road, in lat. 25° 57′ N., and long. 80° 50′ E. The meteor from which the stone was ejected, was of large size, surpassing the full moon in apparent magnitude as well as splendor. It passed from south-east to north-west. A number of stones fell, the largest of which weighed 22 lbs., but that in the possession of Mr. Grant was the only one in an entire state, which was found. It was brought from India by Dr. Tytler, by whom it was presented to its present owner.

The stone is oval, slightly compressed, indented, and possesses a brownish black crust. Its weight is about two pounds. It is fine grained, trachytic, and resembles most closely the stones of Poltawa (March 12, 1811), and of Castine (May 20, 1848). Sp. gr. =3·352.

2. *Charwallas*, 30 *miles from Hissar, India*, *June* 12*th*, 1834.

This is another stone, of which the only notice I have met with is found in the Appendix of the above mentioned work (p. 143),—Prof. Partsch remarking that no portion of the mass had made its way into Europe. The entire stone is in the possession of Prof. Jameson, to whom it had been presented by a relative resident in India at the time of its fall. Its exact weight I am not able to give; but I have the impression that it cannot fall short of 7 or 8 lbs. I owe a fine slice of several ounces weight to the kindness of Prof. Jameson, from an examination of which, I am able to give the following description.

It is one of the toughest stones, if we except those of Chantonnay (Aug. 5, 1812) and Cabbarras Co., N. C. (Oct. 31, 1849), with which I am acquainted. It is filled with iron rust, like certain weathered, fine grained granites, in consequence of which, and the smallness of the particles of composition, it is impossible to recognize the mineral species (with the exception of the nickleiferous iron) of which it is made up, although olivinoid, and one of the feldspar species, appear to be the leading ingredients.

On exposure to the air, it deliquesces, yielding chlorid of iron; but this does not prove chlorine to have been an original ingredient of the

stone, since the mass, as in the case of one of the Iowa (Feb. 25, 1847) stones, may have been since its fall in some situation where chlorine has been imparted to it.

Its specific gravity is 3·38. It contains 15·07 per cent. of nickeliferous iron, with traces of sulphur. The stony part consists of silica, magnesia, protoxyd of iron, alumina, and lime.

3. *Meteoric Iron, County Down, Ireland. Fell Aug.* 10, 5 P. M., 1846.

For a knowledge of this meteorite, I am indebted to my friend Dr. John Scouler, Prof. of the Royal Dublin Institution, who wrote me as follows respecting its fall, in February, 1848. "I believe I must give you the credit of having discovered another meteorite in Ireland, or in other words, but for you I would not have been at the pains of finding it out. The stone or stones fell in 1844, in the north of the county of Down, and were seen to fall by some of the coast guard. You will find two small specimens of this stone along with the other specimens in the box." Owing to an accident in the transmission of the box, the specimens were not received until within a few months, and hence the delay in making known this interesting fall of meteoric iron. The only additional information concerning the event, which I am at present able to communicate, is the circumstance mentioned in the label accompanying the specimens, "that the name of the man who saw the mass fall and who picked it up, was Gibbon."

The following is all that I am able at present to make known concerning the mass. It is malleable, homogeneous, and amygdaloidal. Specific gravity variable: vesicular portions =5·9. Crust thick, sometimes one-third of an inch, and consists of mixed oxyds of iron, somewhat coated by blue phosphate of iron (vivianite). In moist air, the chloride of iron deliquesces in little drops. It does not afford the Widmanstättian figures. It does not contain nickel, cobalt, or sulphur.

4. *Description of a large Stone of the Linn Co., Iowa, Fall of Feb.* 25, 1847.

This stone, weighing twenty pounds, has lately come into my hands through the agency of Rev. R. Gaylord, of Hartford, Iowa, the same gentleman who procured for me the specimens which were picked up at the time of the explosion of the meteor, and of which an account was given at a former meeting of the Association (see Vol. IV., 288, 289, of the American Journal).

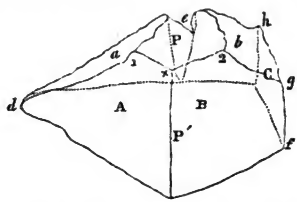
The following statement respecting it is from the Rev. Mr. Gaylord's

letter of July 3d, 1850. "It was found (in the summer of 1847) in Hooshier Grove, by Abner Cox. He was in company with John Hollis, of whom I obtained two fragments three years ago. They have had the stone two years or more, and by lying in the loft of a smoky cabin, it is somewhat dingy in appearance. This John Hollis is the man who ground up so much of the stones that were seen to fall, in order to get silver. He was the means, however, of the careful preservation of the present mass. Dr. Knight found they had the stone, and wrote me respecting it.

"The three pieces into which it broke on striking the ground, fit together exactly, so as to reproduce the original stone, with a complete coating over the whole, except on one side, where several small fragments were broken out by the fall. These were gathered up carefully, and preserved by the finder."

This stone is perhaps the most remarkable one thus far described, for its highly regular prismatic figure, which at once suggests the idea of a portion of a basaltic column. Nor can the geologist look upon it without feeling almost certain that it once formed part of some extensive formation in the world from whence it came.

Its dimensions will be best understood from an examination of the annexed figure. P and P′ are the bases, A, B and C the vertical sides of the prism: 1 and 2 constitute the less regular 4th and 5th faces of the figure. *c* denotes a portion of the stone which is wanting. *a* and



b are sloping sides, *a* inclining to P under an angle of about 130°, and the diagonal of *b* to the line *g f*, under 100°. The surfaces P and P′ are nearly flat, and agree in presenting a peculiar wavy, undulating surface, and a deeper black color than belong to the other faces of the stone, a difference which appears to originate in the nature of the horizontal cleavage of the mass as contrasted with that which is vertical or oblique.

The greatest diameter of the base P′ is $10\frac{1}{4}$ inches.

From	*e* to *f*	measures	$6\frac{1}{2}$	inches.
"	*e* to *d*	"	7	"
"	*e* to *x*	"	$5\frac{3}{4}$	"
"	*g* to *f*	"	$3\frac{3}{4}$	"
"	*g* to *h*	"	$3\frac{3}{4}$	"
"	*x* to 2	"	$2\frac{1}{4}$	"
"	*b* to *g*	"	$4\frac{1}{2}$	"

The fragments which came from the chink *c* are rich in chlorine, deliquescing freely with chloride of iron when exposed to a moist state of the air; while the rest of the stone is quite free from this constituent, and precisely resembles the other stones of the locality already described. This difference of composition in one and the same stone is probably owing to the fact, that the fragments in question must have remained for a considerable time partially buried in the soil, and have imbibed the chlorine from thence; while the main mass being above ground and more protected by its coating, was preserved from such an impregnation.

5. *Meteoric Stone of Waterloo, Seneca Co., N. Y.; fell in the summer of* 1826 *or* 1827.

For my first knowledge of this remarkable stone, I am indebted to Prof. O. Root, of Hamilton College, from whose letter, dated Clinton, N. Y., Jan. 26, 1850, the following abstracts are made. "On receiving your note, I wrote to my friends in Geneva, for the meteorite mentioned in my letter to President Hitchcock. Judge Watkins very willingly gave the specimen, and it is now in my possession, subject to your order. The piece is not large (it weighs about 1000 grs.), as the original mass had been divided two or three times. Not being familiar with such productions, my opinion concerning its genuineness is of no value. Judge Watkins, however, is a gentleman of high respectability, and I have confidence in what he relates of the history of this stone. My attention was directed to the subject in the following manner. A year or two ago, while showing some gentlemen a fragment of the Otsego meteoric iron, one of them observed that he remembered a report many years back, of a stone falling through a roof in Waterloo, or in that vicinity. After many inquiries, I at last found the stone, or a fragment of it, with Judge Watkins. He relates that a hole was discovered in the roof of his mill, directly over a bin of wheat,—that the

opening was made through the shingles where the roof-boards were about five inches apart (although a piece was split from the roof-board on one side), and that under the hole there appeared a depression in the grain, which led to an examination that resulted in the discovery of the stone. The Judge inferred that the stone had fallen through the roof, as its size was too great to have allowed its admission into the bin along with the grain, which was raised by means of elevators. He also supposed it to have been of atmospheric origin, as the mill was four stories high, and as the nature of the stone was unlike any of the mineral productions of the region, the rock in place at Waterloo being the Seneca limestone. He was not positive whether it was found in 1826 or 1827. The stone was divided for Dr. Hale, President of Geneva College."*

The specimen presented me by Prof. Root, had been left for upwards of twenty years in the garret of Judge Watkins, where it appears to have been mistaken for something edible by the rats, who have left numerous markings of their incisor teeth upon its surface. Indeed, in color and texture, it nearly resembles common rhubarb. Its color is light buff or yellow. It is slightly coherent, and may easily be crushed between the fingers. Its sp. gr. =2·30. But a small portion of the original crust remains, which is reddish brown. The stone contains, in small quantity, blackish particles attracted by the magnet. A surface produced by being cut with a saw, shows waved parallel lines of greater hardness than the rest of the stone. It consists of

Silica,	78·80
Peroxyd of iron,	8·72
Alumina,	6·28
Moisture,	4·75
	98·55
Lime and magnesia (in equal quantities), and loss,	1·45
	100·00

6. *Specific Gravities of two Meteoric Irons.*

Meteoric iron of Pittsburgh, Pa.,	7·380
" " Salt River, Ky.,	6·835

* I addressed a letter of inquiry to Dr. H., who informs me that the specimen has for some time been lost sight of in the college collection.

In the course of this communication, Prof. S. exhibited an extremely interesting meteorite, which fell in Linn County, Iowa.

In reply to a question by Mr. Wurtz, as to the chemical nature of the minute yellow specks having metallic lustre interspersed through this stone, Prof. SHEPARD stated that they were iron pyrites. A discussion ensued between Prof. SHEPARD, Dr. JACKSON, Mr. BRUSH, and Mr. BREWER, concerning the presence of chlorine in meteorites, in the course of which it was stated, that although the presence of chlorine might, in some cases, be attributed to accidental causes, yet there were cases in which large quantities of chloride of iron and nickel were found in the centre of the mass, and must, therefore, have belonged to it properly. Prof. Shepard presented copies of his report upon American meteorites to the members of the Section.

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3. ON THE MANUFACTURE OF ZINC AND ZINC WHITE. By DR. C. T. JACKSON.

During the past month I visited the celebrated mine of red oxide of zinc and Franklinite, in Franklin, Sussex county, N. J., and subsequently, by invitation of the proprietors, examined the zinc furnace and paint mills of Newark. I propose to give a brief description of the mine, and of the process of manufacturing zinc white.

The red zinc ore of Franklin and Sterling has been known for many years. It was originally mined by Lord Sterling, anterior to the American Revolution, under the mistaken idea that the ore was red oxide of copper. The discovery of this error caused the mine to be abandoned, and large heaps of the anciently excavated ore now remain upon the ground.

Recently, a few enterprising gentlemen of New York have undertaken to work the mine, with a view to the manufacture of zinc white, that beautiful and unchangeable paint lately discovered and applied in France, as a substitute for white lead.

There having been a regular and systematic opening of the mine effected by the present proprietors, we were enabled to examine the position of the bed, and to obtain good specimens of the ores. The bed is included in white crystalline metamorphic limestone, associated with hypogene rocks of igneous origin. Its trende is nearly north and south, but it follows the curvature of the strata to the westward. The dip of the bed of ore is from 50 to 55° south and eastward. Its

width varies from 8 to 12 feet, but in some places it swells to greater dimensions. On the south side of the hill the ore has been exposed fairly to view by a breast or heading of from 24 to 30 feet in width, and the ore is blasted off like rocks in a quarry, in ponderous masses, which are broken up and transported by wagons to the canal, on which it is transported to the works where it is to undergo the operations I shall presently describe. It is obvious that an inexhaustible supply of the mixed Franklinite and red oxide of zinc may be obtained from this place. And it is to be hoped that it may be advantageously wrought.

Among the minerals for which this locality is celebrated, are large foliated masses of rich red oxide of zinc, crystals of Franklinite, some of which are of large dimensions, Troostite and Foulerite. We observed, also, crystals of apatite of a delicate blue color. White and transparent blende, or sulphuret of zinc, in nests. The pot-hole in the limestone on the east side of the opening of this mine I have described in the Geological Section of this Association. At the old openings made by Lord Sterling, beautiful crystals of Gahnite or zinciferous spinel abound, and I have seen a perfect octahedron of this rare mineral, which was obtained from this place, that was 20 inches in circumference at the base. It is in the possession of Mr. Campfield, at Hon. Mahlen Dickerson's, in Tuccasunny. Cinnamon brown garnets also abound in the veinstone, and large crystals of black mica, Jeffersonite (a variety of Pyroxene,) hornblende, and brilliant white blende also occur.

On our return from the mine we visited the furnace and mills in Newark, N. J.

This furnace was erected for the purpose of manufacturing white oxide of zinc, and the mills are used for grinding the Franklinite and red oxide of zinc.

The furnace will contain 70 horizontal fire clay tubes for reduction and distillation of the zinc, and the apparatus is so arranged as to allow the sublimed zinc to burn at the mouths of the tubes, so that the white oxide is formed at once. This is effected by fixing an adopter to the open end of the subliming tube or retort, and allowing, by a broad slit, the ingress of a current of air. The zinc takes fire, and burns with rapidity, and the flocculent white oxide of zinc is collected in large zinced iron reservoirs, and the fine dust is collected in an upper room by means of tubes with gauze sieves. The retorts or tubes are manufactured at the works from good fine clay, found in the vicinity. Each tube will

receive a charge of about 40 pounds of the mixed ore and anthracite dust at a time. Before the ore is introduced into the retorts, it is first roasted, to render the Franklinite magnetic, and is ground in the mills. It is then freed, as far as practicable, from the Franklinite, by the removal of the latter by means of one of Cook's large magneto-electric separators, which removes the Franklinite, and leaves the red oxide of zinc.

The red oxide of zinc is then mixed with finely ground anthracite in proper proportions for the reduction of the ore, and is then ready for introduction into the retorts.

Not only is the zinc ore used for making zinc white, or metallic zinc, as may be required, but the grouud ore itself is made into an orange colored paint of value. And the Franklinite is ground and used for a brown paint, and also for desulphurizing sulphurous cast iron in the puddling furnaces.

Upon the close of Dr. Jackson's observations, Major A. C. Farrington exhibited specimens of zinc foil, of red zinc ore, and Franklinite, and likewise samples of the oxide of zinc and Franklinite, which had been ground for use as white and brown paints.

Dr. Jackson adverted to the very great beauty of the white paint, and spoke of its superiority over white lead where it was liable to be exposed to sulphuretted hydrogen. The subject drew forth further remarks from Major Farrington, Prof. Shepard, and Mr. Brewer.

Prof. Johnson here resumed the chair.

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4. Analysis of Red Marl of Springfield, Mass. By Dr. C. T. Jackson.

I present a chemical analysis of this rock, which I made for the use of a manufacturer of paint, supposing that it would prove useful to the science of geology, since much interest is taken in the metamorphoses which sandstone undergoes when acted upon by igneous rocks. Its chemical composition may explain some of its reactions on the ingredients of trappean rocks with which it is so frequently combines, forming amygdaloid.

The marl or fine grained sandstone analyzed, was uniform in its structure and composition, so as to admit of a proper analysis. It is fine grained, not gritty—easily crushed, and has a fine red brown color when pulverized. It consists of:

Water,	8·5
Silex,	51·3
Alumina,	16·0
Peroxide of iron, with a trace of manganese,	20·0
Lime,	2·8
Magnesia,	1·0
	99·6

While making this communication, Dr. JACKSON exhibited specimens of wood painted with the ground marl.

Mr. WELLS presented a specimen of the marl itself for inspection, and gave a description of the method of manufacturing it into a paint.

Prof. SHEPARD inquired whether its color would not be improved by burning before grinding it.

Mr. WELLS replied that this treatment rendered it more difficult to reduce it to a fine powder.

Prof. JOHNSON suggested that this substance, when mingled with lime and fused, might form a glass suitable for ornaments.

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5. ON THE DETERMINATION OF PHOSPHORIC ACID. By T. S. HUNT.

This communication was accompanied by remarks upon the analysis of soils, with a statement of some results.

Dr. JACKSON was glad to find that Mr. Hunt had succeeded in bringing the determination of phosphoric acid of soils to such certainty and accuracy. He believed the limits of barrenness and fertility in soils rest within two per cent. in the case of certain of their constituents, and then made some remarks in reference to leached ashes as a fertilizer.

Mr. HUNT thought the limit to lie between much smaller limits than Dr. Jackson had stated.

In this connection Mr. S. W. JOHNSON introduced the subject of the relative value of unleached and leached ashes, maintaining the superiority of the former as an agricultural material. His opinion was founded on the fact, that solutions of potash, as in the case of ammonia, when filtered through soils, were deprived of much of their alkali, and that therefore soils could retain for a considerable period of time the abundance of potash they would receive from the unleached ashes, notwithstanding the action of the rains.

SIXTH DAY, SATURDAY, AUGUST 24, 1850.

GENERAL MEETING.

(*Morning Session.*)

The PRESIDENT in the Chair.

The minutes of the meetings of Friday afternoon and evening were read and confirmed.

Prof. WM. B. ROGERS presented a report of the proceedings of the Standing Committee, comprising various nominations and resolutions.

The Committee on the Prime Meridian were, at their own request, discharged, but the following Committees were continued, viz:

1. On a uniform standard of Weights and Measures.
2. On memorializing the Legislature of Pennsylvania for publication of the Final Geological Report of the State.
3. On the United States Coast Survey.
4. On memorializing Congress in relation to Scientific Explorations.
5. On a change in the Constitution providing for Honorary Members.
6. On Physical Constants.

The following gentlemen were appointed to constitute the Committee on Memorializing the Legislature of Pennsylvania in reference to the publication of the Final Geological Report of the State, viz:—Prof. B. SILLIMAN, Jr., *Chairman;* Prof. EDWARD HITCHCOCK, Amherst, Mass.; SOLOMON W. ROBERTS, Philadelphia; WM. C. REDFIELD, Esq., New York; Prof. A. D. BACHE; Prof. JAMES HALL, Albany, N. Y.

The following gentlemen were added to the Committee on the Coast Survey:—Prof. WM. B. ROGERS, Virginia; Prof. M. I. WILLIAMS, S. Carolina; Prof. CHAS. F. MCCAY, Athens, Georgia; ISRAEL W. ANDREWS, Marietta, Ohio.

The following resolutions, reported by the Committee appointed for that purpose, were read and unanimously adopted, viz:—

Resolved, That in the foundation and maintenance of numerous magnetical and meteorological observatories, the British Government has evinced an appreciation of the claims of science, and a readiness to contribute liberally to its support, which challenge the admiration and demand the hearty acknowledgment of the scientific world.

Resolved, That the experiments which are now in progress at the Toronto Observatory to test the practicability of self-registering photo-

graphic methods—the system of concerted Auroral observations recently organized by Capt. Lefroy—and the peculiar interest attaching to magnetic observations made near the focus of maximum intensity, render it highly desirable that the Toronto Observatory should be continued in activity for a somewhat longer period.

And, inasmuch as a very extensive series of meteorological observations, embracing the entire area of the United States, is now in progress of organization by the Smithsonian Institution, and it would add exceedingly to the value of the proposed observations, if simultaneous ones could be obtained from the region north of the United States, extending even to the shores of Hudson's Bay and the coast of Labrador; therefore,

Resolved, That the British Government and the Directors of the Hudson's Bay Company be invited to co-operate with observers in the United States in united and systematic meteorological inquiries.

The Committee to whom was referred the subject of the establishment of a Zoological Journal in this country, presented the report of the majority of the Committee in attendance, warmly approving the undertaking, and recommending the passage of the following Resolution; and the same was unanimously adopted, viz:

Resolved, That this Association cordially approve of the proposition for the establishment of a Zoological Journal, and recommend the effort to the support of all zoologists, and others interested in the progress of science.

While the foregoing resolution was pending, the senior editor of the *American Journal of Science and Arts* rose to say, that having been for many years concerned in editing a scientific Journal, he desired to express his cordial approbation of the resolution, and the pleasure he should take in seeing the publication of a special journal for this important branch of knowledge.

The PRESIDENT said he wished to bear testimony to the liberality and generous treatment which Prof. Silliman and his associates had always extended towards every effort for establishing, in other parts of this country, periodical works devoted to departments of science included within the range of the *American Journal*.

Prof. AGASSIZ added expressions of the gratification which he experienced in hearing the remarks just made, and in observing the disinterested support and encouragement given to the enterprise by gentlemen with whose interests it might seem to interfere.

The Committee appointed to audit the accounts of the Treasurer, reported that they had examined the vouchers and schedule, and recommended the adoption of the Treasurer's Report. The Treasurer's Report was therefore adopted and approved, and the Committee was discharged.

The following resolution, from the Section of Geology and Natural History, through the Standing Committee, was then adopted, viz.:—

Resolved, That Dr. W. J. Burnett, of Boston, be requested to prepare a report upon the comparison of the parasites of the higher animals of this country with those of Europe.

The following gentlemen were appointed a Committee, to report on the paper of Prof. O. M. Mitchel, on a new method of observing and recording Right Ascensions and North Polar Distances, viz;—Prof. BENJAMIN PEIRCE, Cambridge, Mass.; Prof. J. H. ALEXANDER, Baltimore, Md.; Prof. W. C. BARTLETT, West Point, N. Y.; Dr. HENRY J. ANDERSON, New York; Prof. SAMUEL ST. JOHN, Hudson, Ohio.

It was then

Resolved, That the Standing Committee have power to fix the duties of the Permanent Secretary of this Association.

Resolved, That the Permanent Secretary be a member, *ex officio*, of the Standing Committee.

Resolved, That the Permanent Secretary be instructed to erase from the list of members of this Association the names of all who, by the return of the Treasurer, shall appear to be two years in arrears for annual dues;—suitable notice being given by two circulars from the Treasurer, at an interval of three months, to all who may fall under the intent of this resolution.

Resolved, That Prof. Silliman be requested to close the final General Meeting of the Association, and that the same be held at 3 o'clock this afternoon in the College Chapel.

Resolved, That the Standing Committee have full power to complete and finish any outstanding business of the Association, in their name.

The following gentlemen were appointed a Committee to report on the Elliptic Tables of the planet Neptune, communicated by Prof. Coakley, viz:—Prof. BENJAMIN PEIRCE, Prof. A. D. BACHE, Lieut. CHARLES H. DAVIS.

Resolved, That the Sections be requested to conclude their meetings by one o'clock, P. M.

A communication from Dr. F. LIEBER, received through Prof. HENRY, was referred to the Committee on a Standard of Weights and Measures.

The subject of the right which the authors have over papers presented and read to the Association, was referred to the Standing Committee.

SIXTH DAY, SATURDAY AUGUST, 24, 1850.

GENERAL MEETING.

(*Afternoon Session.*)

The PRESIDENT in the chair.

Minutes of the last session read and approved.

Resolved that the following amendment of the 18th rule of the Constitution be proposed, and that action thereon be taken at the next Annual Meeting, viz.

"The Permanent Secretary shall erase from the list of members the names of all those who, by the return of the Treasurer, shall appear to be two years in arrears for annual dues;—suitable notice being given by two circulars from the Treasurer, at an interval of three months, to all who may fall under the intent of this provision."

Prof. NORTON read the proceedings of the standing Committee.

The following communications were then made:

1. ON THE ESTABLISHMENT AND PROGRESS OF THE ASTRONOMICAL JOURNAL. By Dr. B. A. GOULD.

At the Cambridge meeting of this Association, last year, after a communication by Prof. Hubbard on the advantages which would result from the establishment of an Astronomical Journal, the Section of Physics and Astronomy recommended the undertaking, and appointed a committee to carry it into effect.

The aim of this journal was to be not the diffusion, but the advancement of astronomical knowledge. From the universal conviction which prevails of the importance of the former object, from the efforts which have been, and are still continually making in its behalf, and from the very nature of our American institutions, we may rest assured that the

labors of good men will be continually directed to the support and furtherance of institutions and publications devoted to the diffusion of knowledge in all the arts and sciences. But as regards the advancement of science, members of this Association well know that it is otherwise.

The columns of the Astronomical Journal are designed for scientists rather than for philanthropists. Its province being exclusively the advancement of the science, its design has been rather to aid and serve astronomers, than to interest lovers of astronomy. No communications have been published excepting those sent by the authors, and none have been reprinted from other periodicals, either American or foreign. Astronomers have united with great unanimity in its support, and we may hope that it will not merely serve as a convenient medium for them to make public their investigations, but react, as a stimulus to renewed exertion.

The first number of the Astronomical Journal was published on the 2d Nov. 1849; the fourteenth, Aug. 4th, 1850. The first volume (24 numbers) will probably be completed in the early part of the year 1851.

It cannot, of course, be expected that a publication of this kind can support itself as yet, in a pecuniary sense. For a popular journal of astronomy we might anticipate a very large circulation; but the number of astronomers in the United States is as yet too small to authorize any hope that the income will for several years equal the expenditures. The deficit for the present year cannot be less than $600. That for succeeding years will probably be diminished by the amount of transatlantic subscriptions.

The large expense which is thus necessarily connected with the Journal has offered no obstacle to its commencement and continuance, —thanks to the liberality and love of science of some of our most enlightened citizens. Subscriptions towards supplying the anticipated deficit were made, to a large amount before the publication of the first number, which would not otherwise have been issued.*

I am happy also to announce that by the munificence and public

* Towards the support of the Astrononical Journal, Hon. Edwd. Everett, of Cambridge, Uriah A. Boyden, Esq., of Boston, Prof. A. D. Bache, of Washington, Prof. H. J. Anderson, of New York, and Samuel Appleton, Esq., of Boston, contributed each one hunded dollars; large sums were also contributed by Messrs. J. Ingersoll Bowditch, P. C. Brooks, Nathan Appleton and Wm. T. Bullard, of Boston.

spirit of a single gentleman, the continuance of the Journal through a second volume is placed beyond pecuniary danger.*

In conclusion, I cannot refrain from alluding to the debt of gratitude which the friends of the Journal owe Prof. SCHUMACHER, the eminent editor of the *Astronomische Nachrichten*, the only other publication of the kind which exists, and one which has in a great measure served us as a model. His services were never wanting, where the interests of true science could be promoted, and his aid and friendly counsel have been freely proffered, and gratefully embraced in the establishment of the Astronomical Journal.

After hearing this communication, it was unanimously

Resolved, That the thanks of this Association be presented to those gentlemen, Hon. EDWARD EVERETT, URIAH A. BOYDEN, ALEX. D. BACHE, HENRY J. ANDERSON, SAMUEL APPLETON, J. INGERSOLL BOWDITCH, PETER C. BROOKS, and WM. S. BULLARD, who have so liberally and nobly contributed to the support of the Astronomical Journal.

Resolved further, That the Association recognize, with especial gratitude, the munificence of URIAH A. BOYDEN, of Boston, in also assuming the burden of the deficit of the second volume of the said Journal, and tender to him, in the name of science, their warmest acknowledgments.

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2. ON A SYSTEM OF METEOROLOGICAL OBSERVATIONS ESTABLISHED IN THE STATE OF NEW YORK, BY ORDER OF THE REGENTS OF THE UNIVERSITY, IN CONNECTION WITH AND ACCORDING TO THE DIRECTIONS OF THE SMITHSONIAN INSTITUTION. By Prof. ARNOLD GUYOT, Cambridge.

[*Not received.*]

It was next

Resolved, That Prof. SILLIMAN, sen., be a sub-committee to present to the Provincial Government of Canada, and to the officers of the

* The following letter was received on the 3d inst.

BOSTON, AUG. 3d, 1850.

DEAR SIR,

I propose, if you do not derive sufficient pecuniary aid from astronomers and other supporters of your Astronomical Journal, to defray the expense of publishing the second volume, to contribute myself what money may be wanted to pay the balance of the expenses of publishing said volume. Respectfully,

URIAH A. BOYDEN.

Dr. B. A. GOULD, Jr.

Observatory, a copy of the resolutions respecting the continuance of the Toronto Observatory, and that copies of the same be also transmitted to the Government of Great Britain, and to the Directors of the Hudson's Bay Company.

Voted, That the thanks of the Association be presented to Prof. LEWIS R. GIBBES, for the industry and fidelity with which he performed the duties of Secretary at the meeting of the Association at Charleston, and for the promptness of the publication of the proceedings of that meeting.

Voted, That Dr. ALFRED L. ELWYN be appointed Treasurer of this Association for the year ensuing.

On motion of Prof. OLMSTED, put by the SECRETARY,

Voted, That the thanks of this Association be presented to the President, Prof. ALEXANDER DALLAS BACHE, for the dignified and courteous manner in which he has presided over our deliberations.

The PRESIDENT responded to this vote, expressing his heartfelt thanks for this honor—the satisfaction which it gave him to have won the favorable opinion of a body of men for whom he had such high respect—the grateful recollections he should ever cherish of the season they had spent together, &c.

On motion of Prof. HENRY,

Resolved, That the members of the Association return their warmest thanks to the President, Fellows, and Faculty of Yale College, for the very ample provision made for their accommodation, and for the kind reception extended to them.

This he supported by appropriate remarks, in the course of which he referred particularly to the agency which Prof. SILLIMAN has, during a long career, exercised, in diffusing a love for the cultivation of science.

Prof. OLMSTED expressed, for himself and his associates, the gratification which they had received from their intercourse with the members.

On motion of Prof. B. SILLIMAN, jun.,

Resolved, That the thanks of this Association be presented to the Secretary, Assistant Secretary, and the Secretaries of the several Sections, for the able manner in which they have performed their laborious duties.

On motion of Prof. B. SILLIMAN, jun.,

Resolved, That the thanks of this Association be extended to the Committee of the College Street Ecclesiastical Society, for their liberality in granting the use of their beautiful house for the evening meetings of the Association during the present session.

On motion of Prof. J. B. ROGERS,

Resolved, That the members of the Association return their warmest thanks to the ladies of New Haven, who, by their presence, have animated, as they have adorned our meetings, and given additional incitement to the future labors of its members.

On motion of Dr. J. H. GIBBONS,

Resolved, That the hearty thanks of the Association are due, and offered to the citizens of New Haven, for the very hospitable reception which they have given the members, and for the interest which they have manifested in the proceedings.

On motion of Dr. A. L. ELWYN,

Resolved, That a copy of the printed volume of the proceedings of the meetings at Philadelphia, Cambridge, and New Haven, be presented to the libraries of Harvard and Yale.

In pursuance of a resolution passed at the meeting in the morning, the PRESIDENT here invited Prof. SILLIMAN to address the members, and thus bring the session to a close.

In the remarks which he offered, Prof. S. communicated a sketch of his personal history as connected with the early science of the country, and presented some account of the rise and progress of the Association. He discoursed also on the objects and duties of the Association, and on the harmony which must ever exist between scientific investigation and religious truth. Then, tendering his kind wishes to each, he bade farewell to all, and the meeting ended.

SIXTH DAY, SATURDAY, AUGUST, 24, 1850.

SECTION OF GEOLOGY AND NATURAL HISTORY.

(*Morning Session.*)

This Section met at 10 A. M. in the Geological lecture room of Yale College, and was called to order by Dr. STEPHEN REED in the chair.

The minutes of the last meeting were read by the Secretary, and approved. The following communications were then received:

1. NOTICE OF THE HABITS OF PLOIARIA BREVIPENNIS. By Rev. THOMAS HILL. Read by Prof. AGASSIZ.

WALTHAM, Aug. 14, 1850.

MY DEAR SIR:

You asked me, some months since, to give you in writing my observations on the Ploiaria brevipennis, and I choose the present time for complying, because I think the letter may seem to you appropriate to be read at the A. A. A. S. in New Haven.

I first used to see them in 1825–29 at my father's house, New Brunswick, N. J., always walking on the under side of the barn roof; except the first time, when I saw them flying across the barn loft.

In 1846 I saw one flying at dusk in Waltham.

In Oct., 1848, I showed you a picture of one; you pronounced it rare or unknown, and I have ever since sought opportunities to observe them, and made notes of what I saw.

On the 28th of August, 1849, I found abundance of them in my meeting-house horse sheds, just undergoimg the last transformation and coming out winged.

In the latter part of Sept. and first of Oct. I saw many couples in coitu. The males are distinguished readily by brilliant red spots on the back and upper side of abdomen.

On the 28th of June, 1850, I found numbers of them in two different localities; quite young, measuring scant a quarter of an inch.

They appear now full grown and ready for final transformation, being nine eighths of an inch in length, (exclusive of raptorial legs.)

In regard to their habits. They frequent the spider webs from their infancy, and seldom get entangled. When slightly entanged, they free themselves by using their raptorial legs as hands, which they can do very adroitly by bending themselves in very odd positions to get their

hands to the entangled member. Sometimes when cobwebs get on the hind legs, they tangle it on a splinter of wood, and then lying flat, drag themselves forward by the raptorial legs. When very badly entangled, they are sometimes attacked and conquered by the spider.

They live principally on flies, and take their prey by stealth, but instead of approaching like the hornet and the hunting spider in the rear, they prefer, I think, to approach in front. They approach very slowly, but are not afraid to ascertain the fly's position more accurately by feeling with their long antennæ. When they get into the right position, they raise the raptorial legs very slowly over their heads, and then bring them down upon the fly with an instantaneous movement. The first one that I saw strike a fly, stood exactly facing it, and brought his raptorial legs, with a sudden blow, down between the fly's wings; parting, dislocating and holding them still, and at the same time lifting the fly off its feet and thrusting his beak into its back;—a most admirable instance of the perfect harmony between the instincts and the formation of creatures.

Yours, very truly,

THOMAS HILL.

The insects are most active at dusk, or on cloudy days.

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2. On motion of Prof. L. Agassiz, the paper entitled "Description of a new species of Helix, by Samuel J. Parker, Ithaca," was presented out of its regular order, and read by Prof. C. B. Adams.

Prof. Agassiz, after complimenting Mr. Parker on the zeal he manifested, commented on his misfortune in describing a species as new, without previous comparison to see whether this were really the case. Such a course was becoming too common. It showed zeal, but not *criticism;* and without critical examination, no authority can attach to any name.

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3. On Fossil Species of Walrus found by Prof. Fraser on the Shores of New Jersey. By Prof. Louis Agassiz.

[*Not received.*]

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4. NOTICE OF OBSERVATIONS ON DRIFT STRIÆ IN NEW BRUNSWICK. By PROF. JAMES ROBB, Fredericton, N. B.

No.	*Locality.*	*Rock Scratched.*	*Dir. of Scratches by compasses.*
1	Near St. John at Brickyard,	Slates, . . .	N. & S.
2	" " at Penitentiary,	" . . .	N. 30° E.
3	" " at South Bay,	Syenite, . . .	N. 25° E.
4	" Mouth of Nerepis R.,	" . . .	N. 45° W.
5	" Ox-bow bend of Ner. R.,	" . . .	N. 30° W.
6	In Nerepis Settlement,	Slates, . . .	N. 10° W.
7	Near Oak Bay,	" . . .	N. & S.
8	At St. Andrew's Seashore,	Red sandstone, .	N. 10° W.
9	At Chamcook Lake,	Trap, ...	N. 30° W.
10	Near St. Andrews on upland,	Red sandstone, .	N. 10° W.
11	At L'Etang Harbor,	Trap, ...	N. 45° W.
12	Bet. St. George's & L'Etang,	Slate, . .	N. 45° W.
13	At Falls of Nagaguadaire R.,	Trap, . . .	N. 46° W.
14	At Musquash Mills,	Slates, . . .	N. 18° W.
15	East of Musquash R.	Granite, . . .	N. 20° E.
16	At St. Mary's, near Fred'n.,	Gray grits, . .	N. 10 & 15° W.
17	Four miles N. of Fredericton,	" " . .	N. 10° W.
18	Near Maryland, &c. &c. &c.,	" " . . .	N. 10° W.
19	At Dyer's on Harwell Road,	" "	N. 10° W.
20	Old Woodstock Road,	" "	N. 10° W.
21	At Spring Hill,	Slates, ...	N. 10° W.
22	Near French Village,	"	N. 10° W.
23	Hill beyond Indian Village,	"	N. 10° W.
24	Near Naylis' Royal Road,	Gray grit, ...	N. 15° W.
25	At Cardigan Settlement,	" "	N. & S.
26	At Rushagonish Bridge,	Purple grits, . . .	N. 10° W.
27	South end of Oromocto L.,	Gray grits, . . .	N. & S.
28	At Hunter's Ferry, Quaco L.,	" " . . .	N. 12° W.
29	At Bupel's Cone, Grand L.	" Sandstone, .	N. 10 W.
30	In Harney Settlement,	Silicious conglomerate	N. & S.
31	Near Hardrig's on Nerepis R	Gr'stone & porphyry,	N. & S.
32	Old Mill near Gagetown,	Reddish sandstone,	N. 22° W.
33	At Harwell Schoolhouse,	" " . .	N. 10 & 45° W.
34	Near Oromocto L., W. end,	Gray grits, . . .	N. 30° W.
35	Mouth of Keswick R.,	White conglomerate,	N. 30° W.

36	Near Gillon's Tavern,	Gray sandstone, .	N. 10° W.
37	Two miles S. of Gillon's,	" " . . .	N. 10° W.
38	Near Madawaska Chapel,	Slates, ...	N. 40° W.
39	Cocagne Beach,	Gray grits, . . .	N. 45° E. & E. & W.
40	In Prince William Parish,	" " . . .	N. 10° W.

As the magnetic variation in New Brunswick ranges from 10° to 20° W., we may from the foregoing observations conclude that the normal direction of the drift striæ in the tract of country alluded to is from about 10° W. of true north to 10° E. of south, or very nearly N. and S.; and that the occasional deviations from the above course may be accounted for by the influence of the adjacent hills and valleys upon the moving materials by which the striæ were produced.

The polishing and scratching of rocks in New Brunswick would seem to be universal; the above are a few out of many hundred cases which I have seen. They are observable on every rock whose texture would appear capable of receiving or retaining fine lines. The phenomena occur at all levels, and in one case appear distinctly upon a limestone on the sea shore between high and low water mark, where the dashing of water, ice, and stones from the beginning of the historic period, has not sufficed to obliterate these deeply graven monuments of diluvial action.

The foregoing data have been offered to this Association, not from any ideas as to the novelty of their results, but simply that they may be added to the already large accumulation of facts bearing upon general history of North American drift; and I would venture to suggest that no more interesting object could be proposed to this Association than the graphical delineation of all those details which we now possess concerning this subject. When the eye can trace upon a map the various courses of the drift striæ, we shall then be able to investigate the dynamical agencies by which they were produced and modified, with as much care and certainty as we can trace the course of the tidal currents in Boston Harbor upon the charts recently submitted by our learned President the Superintendent of the United States Coast survey.

Dr. S. Reed inquired whether the hills give any fixed direction to the striæ; and also, whether those striæ on the higher hills have more deviation to the east than those on the lower. The work had been done at different periods in this country. He thought he had observed

a direction on the hills differing from that in the valleys. In the latter he had traced the striæ more to the South, S. 25° to 35° E., while on the hills he had found the direction S. 45° E.

Prof. ROBB considered the general direction 10° W. of N.; but had not paid any special attention to the point now raised, though he had in mind one or two places where the direction was the same on the hills and in the valleys.

Prof. AGASSIZ pointed out the importance of *mapping down* the direction of the striæ. The agency he would not discuss; but would ask how the appearance of the rocks of New Brunswick compared with those of Europe, which were rough on the north side and polished on the south.

Prof. ROBB thought they presented the same appearance.

Prof. AGASSIZ explained the difference between the action of a solid body and drift; and observed that notice should be taken which side was rough, and which smooth and scratched.

Prof. ROBB had avoided discussing the agency; he had spoken of the direction of the force, rather than calling it either ice or drift. He would say farther, that he had noticed striæ on the gray sandstone at Cocagne, as well as on other rocks.

Prof. AGASSIZ thought the observation of striæ on so many different rocks was very important, as this fact showed distinctly, that the opinion put forward in Europe by no less a man than Leopold von Buch,—that superficial scratches belong to the structure of the rocks themselves,—is not correct.

Dr. S. REED had observed that the steep side was rough, while the gradual slope was ground most thoroughly. The striæ had sometimes gone over on the S. W. extremity, but it was the North and West that had taken the scratching, as he had noticed to a very considerable extent.

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5. ON A LOCALITY OF ASPHALTUM, AND ITS ORIGIN. BY. T. S. HUNT.

[*Not received.*]

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6. Comparison between the young Caterpillars of Lepidoptera and the adult Larvæ of Mosquitoes and the Mode of Formation of Stigmata. By Prof. L. Agassiz.

[*Not received.*]

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7. On some Localities of Magnesite, with Remarks on its Connection with the Origin of Serpentine. By T. S. Hunt.

[*Not received.*]

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8. On the Cylindrical Structure observed in Potsdam Sandstone. By Franklin B. Hough, M. D., Somerville, N. Y. Read by Prof. C. U. Shepard.

A frequent and very interesting phenomenon is observed in the Potsdam sandstone of Jefferson and St. Lawrence counties, N. Y., and consits in the occurrence of *vertical cylinders*, often of great size, and of undetermined length, having a concentric stratification, but not differing in color or texture from the surrounding rock.

The diameters of these masses vary from two inches to twenty feet and upward, but the concentric lines are more conspicuous in those of from one to two feet in thickness, in which the resemblance to the lines of growth in an exogenous tree is so conspicuous, as to have led to the opinion among casual observers, that they were fossils, and hence they have received the name of "petrified logs."

The immense size which they sometimes attain, and especially the age and character of the rock in which they are imbedded, of course preclude the possibility of this, and we are led to seek for their origin in the causes which may have operated at the period when the rock was deposited.

It is frequently observed that the larger masses contain within them those of a smaller size, which are often excentric to the larger cylinder, and sometimes partly within and partly without it. When this occurs, the strata of the smaller mass are entire, while those of the larger one have their course interrupted at the place occupied by the smaller. The length which they attain has not been satisfactorily determined, as the rock in which they occur does not possess that cleavage and system

of joints which sometimes render the Potsdam sandstone a very desirable building material. It is therefore seldom quarried, and but casual opportunities have been presented for measuring their length. Cylinders a foot in diameter have been observed, five feet in length, without change of size or structure, and there is reason to believe that the larger ones extend to a great depth.

A conical figure is occasionally observed in detached specimens, and wherever such a form is seen *in situ*, the vertex of the cone is directed downwards. In no case has the reverse been noticed, nor has any other than the vertical direction come under observation.

In the strata associated with this structure, there is often exhibited the appearance of ripple marks upon the surface of the rock, and upon the faces of slabs of the stone that is quarried. A spheroidal structure is also often met with in the vicinity. The latter consists of numerous balls, seldom larger than an orange, and usually occurring together in great numbers. When broken, they present a very perfect concentric stratification; the laminæ being of different colors, and frequently a pebble in the centre serves as a nucleus. No trace of organic life is observed in the rock which furnishes these curious structures.

The spheroids were evidently formed by the rolling of small bodies along the sandy bottom of an ocean, where they were thrown together into piles, by eddies and currents; while the ripple marks indicate in like manner a fluctuation and unstable stratum of sand, at no great depth below the surface of the water.

When we consider the great mobility of sand when immersed in water, as is exhibited in the familiar example of quicksands, we may readily infer that the cylindrical masses above described may have had their origin in small eddies, or whirlpools, produced by local causes acting upon the surface of the water, and transmitted to the sand at the bottom. The conical figure occasionally observed, and more especially the uniform direction of the vertex of the cone, confirm the idea, while all of the attendant appearances unite to prove that they were produced in shallow water.

In a few instances the surfaces of rock which present sections of these cylinders exhibit evidences of the progressive motion of the vortices which produced them.

When two are found occupying the same area, it is evident that they were formed at distinct, although not probably at distant, intervals of time.

In conclusion, it is interestiug to remark the lasting effects of apparently trivial causes.

The slightest ripple which breaks upon the beach of the ocean, or the eddy which curls upon its surface, may disappear with the moment that produced it, but its history remains in imperishable characters, and after the lapse of countless ages we may trace its directions, estimate its force, and reason upon the causes that formed it.

Prof. Agassiz suggested that another cause had produced this effect, —the plutonic agency. He also referred to the frequency with which metamorphic rocks are met in the Potsdam sandstone.

Prof. Shepard objected to the view of a plutonic agency, from the absence of angles.

Prof. Agassiz thought, however, his view ought to be present in the mind of the investigator.

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9. Remarks on the Geology of Mackinac, Drummond and St. Joseph's Islands, and the Northern Shores of Lake Michigan. By Prof. James Hall. Read by Prof. Agassiz.

[*Not received.*]

Prof. Agassiz made a few remarks on the importance of these surveys.

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10. Remarks on the Seventeen Year Locust. By Miss Morris. Communicated by Prof. Agassiz.

[*Not received.*]

Dr. J. H. Gibbons inquired whether eggs were ever found deposited in the limbs of trees.

Prof. Agassiz replied that they were; and that after the eggs were hatched, the larvæ drop to the ground.

SIXTH DAY, SATURDAY, AUGUST 25, 1850.

SECTION OF MATHEMATICS AND PHYSICS.

(Morning Session.)

Prof. COAKLEY was called to the chair, and the following communications were read or presented :—

1. ON THE USE OF THE ZENITH TELESCOPE IN THE DETERMINATION OF LATITUDES. By Prof. LEWIS R. GIBBES, of Charleston, S. C. Presented by Prof. A. D. BACHE.

Accompanying this letter, you will find my form* for recording observations with the Zenith Telescope, the specimens being taken from my record book of actual observations: three pairs are sufficient to show the method. The fourth column contains the record of the micrometer measures, + for the southern stars in the instrument used; the algebraic sum of these gives the Diff. Zen. Dist. with its proper sign, which is written in the fifth column on a line with the second star; the value of this taken from a table for the micrometer is written beneath, with the same sign. In the sixth column are written the North Polar Distances of each star, on a line with the respective star, and their sum for the pair on the line below. The seventh column contains the Level Readings, which I have always marked N. and S. (rejecting O. and E.) S. always +; half the algebraic sum of these readings for each star is written with its proper sign in the eighth column; the values of these from a table for the divisions of the level is put in the ninth column with their signs, and their algebraic sum in the line beneath, with its proper sign. In the tenth column is the Meridian Distance, + for southern star, and of the reductions to meridian obtained as presently to be stated, the algebraic sum is set down in the line below, with its proper sign. In the eleventh column, in the line below the second star, is written the Difference in Refraction at the Zenith distances of the two stars, obtained as presently to be shown—if southern star be farthest from Zenith. When this is done, the sum of the polar distances, the diff. Zen. dist. and the three corrections for level, mer. dist., and refraction, will be found on the same horizontal line, with their proper signs; their algebraic sum is then taken and written in the twelfth column, on the same line; this is the double co-latitude. At the end of any interval

* See page 358.

of observations, the sum of these may be obtained, and the mean taken; the half will be the co-latitude; the complement is, of course, the latitude sought.

Thus you see the record and the steps of the computation are brought within the space of three lines and twelve columns for each pair, and all of my observations are reduced in this way, on the same sheet on which they are recorded. This is the smallest number of horizontal lines into which the whole process can be brought; but by using four horizontal lines to each pair, two vertical columns may be saved, and only one reference to the table for level need be made, as shown in example.

DATE.	STAR B.A.C.		Microm. Reading-Diff.Z.D. in Arc.	Star's North Polar Distance.	Level Reading and Sum. S. N.	Half sum and correc'n	Meridian Distance and correction.	Refr. correc'n	DOUBLE CO-LATITUDE.
1849. Feb. 24.	1528	S	+23·102	65 11 26·25	+ 47·0—44·5		+ 2 sec.		
	1541	N	—16·100	49 08 62·98	+ 46·0—45·5		— 0		
			+ 7·002		+ 93·0—90·0	+ 1·50			
			+5·20·05	114 20 29·23		+ 2″·38	— 0″·00	+ 0″·16	114° 25′ 51″·76

The only objection to these modes of recording and computing, that occurs to me, is this; the differences from the final mean are twice as great as in your mode, and the squares four times as great, and therefore more cumbrous to use in obtaining the probable errors, but it is easy to halve these differences, and use them as before.

To obtain the correction for refraction, I proceed thus. As the difference of refraction at the two zenith distances is required, and not the absolute amount, I differentiate the formula for refraction, and for this purpose any one of those given will answer Take Bradley's $r = 57''$ tan z; $dr = 57''$ sec ${}^2 z dz$, put $dz = 1'$, $2'$, $3'$, &c., successively, and for each, $z = 0°$, $5°$, $10°$, &c., and we have dr, as in table subjoined, as a specimen. It is easy to obtain the correction by inspection or by multiplication.

z	$dz = 1'$	$2''$
0°	0″·017	·034
5	·017	·035
10	·018	·036 &c.
15	·018	·037
20	·019	·040
25	·021	·042

The correction for meridian distance is not so easily obtained, since it depends on latitude, zenith distance and time, both the last being variable. The expression for the reduction to the meridian, taking only the first term, is Red. $= Ay$; where

$$A = \frac{2 \sin^2 \frac{1}{2} t^{Sec}}{\sin 1''} = 0''\cdot0005454 t^2,$$

and $y = \frac{\sin\lambda \sin\pi}{\sin z} = \frac{\sin\lambda \sin(\lambda + z)}{\sin z} = \sin^2\lambda \cot z + \sin\lambda \cos\lambda$;

λ being colatitude, π north polar distance, and z the zenith distance, — when north. Put $0''\cdot 0005454 = c$ and reduction to meridian $= M$; then $M = Ay = ct^2 \sin^2\lambda \cot z + ct^2 \sin\lambda \cos\lambda$ for south star, and for northern star $-M_1 = -ct_1^2 \sin^2\lambda \cot z + ct_1^2 \sin\lambda \cos\lambda$, taking $z =$ mean zenith distance of two stars. Hence

$$M - M_1 = (t^2 - t_1^2)\, c \sin^2\lambda \cot z + (t^2 + t_1^2)\, c \sin\lambda \cos\lambda.$$

Two tables can be constructed for 1^{sec} to any required number of seconds, one for $c \sin\lambda \cos\lambda$, whose value for my latitude and for 1^{sec} is $0''00024867$. and is only perceptible when the sum of the squares of the seconds of meridian distance is fifty seconds or more; the other for $c \sin^2\lambda \cot z$, for 1^{sec} to several, and for values of z from $0°$ to $25°$.

z	1^{sec}.
$0°$	inf.
$1°$	$0''\cdot 009165$
$5°$	$0\cdot 001826$
$10°$	$0\cdot 000906$
$15°$	$0\cdot 000596$

A specimen for my latitude for 1^{sec} is subjoined. As cot z enters into this expression, it will be seen that the sentence on page 8 of Lee's paper: "Those stars near the zenith being of course preferable," must be taken with some limitation. In computing this last table, this remark may simplify the calculation in some parts,

$$\cot\chi = \frac{1}{\chi} - \frac{\chi}{3} - \frac{\chi^3}{3^2\cdot 5} - \&c.$$

Hence for small arcs the cotangent is inversely proportioned to the arc, or, more accurately, is equal to the reciprocal of the arc diminished by a third of the arc.

You desired to have some account of the mode of recording and reducing, which you saw me using, and I must beg you to accept this, imperfect as it is, and do what you please with it—incorporate it in your own method, or reject it altogether. There are tastes in these things as well as in the fine arts.

UNITED STATES COAST SURVEY.

LATITUDE STATION—CHARLESTON OBSERVATORY. LAT. 32° 47′ N., LON. 5 h. 20 m. W. OBSERVATIONS WITH ZENITH TELESCOPE No. 1.

DATE.	STAR B. A. C.		MICROMETER 120ths REV. DIV.	DIFFERENCE Z. D. BY MICROMETER IN ARC.	STAR'S NORTH POLAR DISTANCE.	LEVEL READING. S. N.	HALF SUM.	LEVEL CORRECTION.	MER. DIST. AND COR'N.	REFR. CORR'N	DOUBLE CO-LATITUDE.
1849. Feb. 24.	1528	S	+ 23·102		65° 11′ 26″ ·25	+47·0 —44·5	+ 1·25	+ 1″ ·98	+ 2″		
	1541	N	— 16·100	+ 7·002	49 08 62 ·98	+46·0 —45·5	+ 0·25	+ 0″ ·40	— 0		
				+ 5′ 20″·05	114° 20′ 29″ ·23			+ 2″ ·38	0″ 00	+ 0″ ·10	114° 25′ 51″ ·76
"	1828	S	+ 19·049		72 20 07 ·81	+48·5 —43·0	+ 2·75	+ 4″ ·35	+ 3″		
	1902	N	— 20·113	— 1·064	42 06 54 ·20	+43·5 —49·0	— 2·75	— 4″ ·35	— 1″		
				— 1′ 09″·91	114° 27′ 02″ ·01			0″ ·00	0″ 00	— 0″ ·02	52″ ·08
"	2200	N	— 16·063		46 16 49 ·04	+45·0 —48·0	— 1·50	— 2″ ·37	— 1″		
	2233	S	+ 23·002	+ 6·059	68 04 06 ·50	+47·0 —45·5	+ 0·75	+ 1″ ·19	+ 9″		
				+ 4′ 56″·02	114° 20′ 55″ ·54			—1″ ·18	+0″·18	+ 0″ ·09	56″ ·65

Sum, ...	4″ ·49
Mean, ...	114° 25′ 51″ ·49
Co-Latitude, . . .	57° 12′ 55″ ·75
Latitude, ...	32° 47′ 04″ ·25

2. On the Law of Induction of an Electrical Current upon itself, and of Electrical Discharges in Straight Wires. By J. H. Lane, Patent Office, Washington.

The first discovery of the induction of electric currents was made by Prof. Faraday. After his discovery, Prof. Henry followed, in a very elaborate series of experiments; in which he developed some new and remarkable phenomena, and brought out the most complete elucidation of the laws of the induction of electric currents that has been produced. He discovered the induction of an electric current upon itself, and referred it to the general principle of inductive action at a distance, as discovered by Faraday.

The conductor of a current was regarded as made up of an infinite number of infinitely small fibres, and the induction of the current upon itself was no more than the inductive force exerted upon each fibre, by the currents generated in all the other fibres.

My object in this communication is to present the principal results of a mathematical investigation of the laws of the induction of a current upon itself, some account of which was read before a meeting of the National Institute, in Washington, some two or three months since.

The most material part of the fundamental law from which we must set out in such an investigation, has been proposed by Prof. Henry, as a deduction from his investigations above alluded to. His expression of the law is not quite complete, but requires, to make it so, only two or three assumptions, which are too plainly suggested by the analogy of other well established laws of electricity to admit of much doubt. Still, they cannot be regarded as demonstrated by actual experiment; and if the results here given, as deductions from them, shall furnish any means of testing their truth, one object of the investigation will have been attained. The fundamental law, then, adopted as the basis of the investigation, is this:—

The inductive force exerted by a current generated in an infinitely short element of a linear conductor, at any given point, lies in the plane of the elementary current, in an opposite direction to it, and at right angles to the line of direction drawn from it to the given point; and is directly proportional to the length of the element multiplied into the sine of the angle it makes with the line of direction, and inversely proportional to the square of the distance.

As it is my intention to publish my investigation in full in the *American Journal of Science,* I forbear, at this time, any further comment on

the fundamental law here stated, or anything more than a brief statement of the deductions I have made from it.

The first of these is the obvious one, that the inductive force exerted by a current generated in a straight linear conductor of unlimited length, is in a direction parallel to the conductor, and in the inverse ratio simply of the perpendicular distance from it. The same is very nearly true with a conductor of limited length, for any point whose perpendicular distance is small compared with the distance from either extremity of the conductor, the error being only a small quantity of the third degree. That in fact the inductive force of such a linear conductor cannot be inversely as the square of the distance, at least not without limitation as to the smallness of the distance, is proved by the fact that the induction of a current upon itself would, in that case, be infinite, so that it would be impossible to generate a current.

In the second place, it will be readily seen that, if a current of electricity were generated in a long, straight, prismatic conductor, in such manner that the rate of development should be uniform throughout the mass of the conductor, the whole inductive force exerted by the entire current would be greater in the central parts of the cross section than at or near the surface. In the case of a cylindrical conductor, the inductive force exerted at the surface would be to that exerted at the centre, as the diameter of a circle to half its circumference.

At the first instant, therefore, of the application of an electro-motive force to generate a current in a straight, cylindrical wire, we must expect that the rate of development of the current will be more rapid in fibres situated near the surface than in the more central ones, in order that the inductive force may be the same in all the fibres, and just sufficient to balance the electro-motive force applied.

I have therefore sought, in the third place, the law according to which the rate of development must vary at different distances from the centre of the conductor, in order to satisfy this condition of equal inductive force through all parts of the cross section, and the result obtained is, that the rate of development, in any fibre, is inversely as the length of the shortest cord that can be drawn through that fibre, in the cross section of the wire.

An analogous law would also obtain, with a straight conductor, having its cross section an ellipse. If a diameter be drawn in the cross section, through any fibre, and a second diameter conjugate to it, the rate of development in the fibre must be directly as the conjugate diameter, and inversely as the cord drawn parallel to it through the fibre.

This law of development can only apply, however, to the incipient stage of the development, since the resistance to conduction in the wire would interfere with it as the current increased in quantity.

In the case, however, of the discharge of machine electricity through a wire, there are strong reasons for believing hat the current, in most cases, never reaches a quantity sufficient to make the resistance to conduction much felt, in comparison with the immense electro-motive force concerned, and to which the inductive force itself + the resistance to conduction, must be equal. If this is found to be actually the case, the law of discharge of machine electricity will depend principally upon the induction of the current upon itself; and the above law of development, or the more comprehensive formula from which it is taken, lead to some curious results in regard to electrical discharges.

That which at once arrests attention is the tendency of the discharge to the surface of the wire, presenting some analogy to the distribution of statical electricity. But the discharges would be far from being confined to the surface; for though the initial development would become infinite at the very surface, yet the depth to which this extends is an infinitessimal of the second degree, so that the discharge must be distributed throughout the mass with a large excess near the surface.

Another interesting result is, that, if the discharge were passed successively through two wires of different diameters, placed end to end, the induction of the current upon itself in the two wires, or what might be called the resistance to discharge, would be directly as the length and inversely as the diameter of the wire, instead of the area of the cross section. This, should it be confirmed, would appear like a marked distinction between galvanic and machine electricity, were we unacquainted with the induction of electric currents.

I am anxious to put these conclusions to the test of experiment as soon as I have opportunity; but the very limited apparatus and means at my disposal have not, thus far, enabled me to produce any reliable results.

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3. On Whirlwinds produced by the Burning of a Cane-brake in Alabama. By A. F. Olmsted, New Haven.

Facts relative to whirlwinds produced by the burning of brush and timber on several occasions, were published in the thirty-sixth volume of the *American Journal of Science*. In the several accounts given in

that article, there is a great sameness, from the fires having the same origin, and differing only in their magnitude. The facts which I shall present to the Association at this time, are different from any that have been recorded on the subject of whirlwinds, and will, therefore, have the interest of novelty, although in magnificence they are not equal to those described in the article alluded to.

The canes in the cane-brakes of Alabama often grow to the height of thirty-five or forty feet. They cover the best of the new land, and the thickness and size of the cane is considered as the best evidence of the fertility of the soil. No vegetation at the North can be compared with the cane-brakes in the density of their growth, and even in the Southern States, no other kind of vegetation is comparable with the canes in this respect. Notwithstanding their great height, they stand but an inch or two apart. A thick cane-brake, therefore, or one in rich land, which is not exposed to the continual intrusion and trampling of large animals, is completely impassable. Such cane-brakes are the resort of a great variety of small animals, which make their way to their coverts through hidden paths. These animals, when the canes are burnt, are overtaken and destroyed by the flames. The canes terminate in a head of foliage in the form of a wide broom or brush, with leaves very much like those of the oleander.

From the dense manner in which the canes grow, it follows that, when cut down, they lie in heaps of great size, and in such a way that when perfectly dry, the fire soon catches the whole mass. They are cut down, generally, with a carpenter's adze, although an axe or a very heavy kind of hoe, called a "cane hoe," is sometimes employed. One blow is sufficient to divide the stalk. This the laborer takes hold of with his left hand, and throwing it behind him, advances to the next. In this way an acre of cane-brake is soon cleared, and cane land is preferred to all other kinds of land for its good soil, and the ease with which it may be cleared. When the canes are cut down, they are allowed to lie for a month or six weeks, until they become perfectly dry. Fire is then applied in several places at once. As soon as the canes begin to burn, the air that is confined in their cells, and the watery vapor, burst them asunder. They generally explode through several cells at once, and thus are split in one continued line. These explosions, in burning a large cane-brake, produce a continued roar, like the discharge of musketry from an immense army. The "burning," however, was unattended with thunder, and, in this respect, differed from all the cases mentioned in the *Journal of Science*. On account of

the dry, combustible nature of the cane, when kindled, the fire advances with great rapidity, giving out flames of the deepest red, the intensity and richness of which color are incomparably finer than the flames which arise from the combustion of any other kind of wood. The deep, rich tints of the dahlia, and some other flowers, approach more nearly to the color of this flame than any other natural object. The finest red of the prismatic spectrum, when all the other rays are cut off, is not superior to it. Together with the flame there ascends a very dense, black smoke, resembling that which arises from burning camphor, or from the chimneys of gas works of factories where bituminous coal is used. This smoke also far surpasses, in its dense deep black color, anything ordinarily observed. The smoke from bituminous coal would not equal it in density, and would be far inferior in depth of color. To the painter, the magnificence of the rich red flame, combined with the rolling masses of smoke, would form one of the finest objects he could select. His darkest color for the smoke, and richest tints for the flame, would be required to convey any just conception of the beauty of this scene.

The cane-brake which I visited covered a space of twenty-five acres. It was set on fire at the part most distant from us, and the tract of land being somewhat curved and irregular, the fire was at first hid from our view. The smoke, however, was visible from the commencement, and the roar of a thousand discharges approached us with rapidity, until, with the flame and the smoke, it issued from behind the wood by which it had been for some time concealed. The heat became intense where we stood, although at a distance of more than two hundred yards from the fire. Whirlwinds were now observed in the hottest part of the fire. They did not unite in one column, as in all the cases mentioned in the *Journal of Science*, but were scattered throughout the fire, and several were formed at the same time.

The first were on a comparatively small scale. Their height was from thirty to forty feet. To these succeeded others on a larger scale, until they reached the height of more than two hundred feet, and the flame and smoke which formed their columns were perfectly distinct from the general mass which arose from the fire. These continued to form until we left. While we remained, there were as many as four or five of the largest size formed. They appeared rather to increase in size and frequency toward the latter part of "the burning," and many were formed on the ashes, after the fire had, to a great extent, gone down. The "burning" lasted for about half an hour.

Among the whirlwinds, there were several points of difference, by which they might be classed under four heads. The most common whirlwind was that which was stationary over a part of the fire which was hotter than the neighboring portions. A second variety was that which had a progressive motion, and advanced over the burnt track, throwing up ashes and cinders, and thus marking its course through the fire. Some of these emerged from the flames. This was probably the case with quite a number, although, having nothing to mark them after leaving the fire, they became invisible. One, however, passed near enough to us to be observed, and attracted our attention by its rustling sound, and by the leaves which it carried up. This was about fifteen or twenty feet high. At the time this passed us, we had moved from our first station, and were about three hundred yards from the fire. These whirlwinds differed from the others in form, being very wide at the top, and contracting to a point at the bottom, like a top or a spindle, or, more exactly, they were the form of the upper cone of an hour glass. An interesting phenomenon which attended some of the whirlwinds, might render it proper to arrange them under a third class. In these the flame was violently whirled at the base; then above succeeded a dark interval, where the flame seemed to be extinguished entirely, but towards the top it broke out anew. It was a mixed whirling of flame and smoke, the smoke occupying the central portion. The dark interval where the smoke was unconsumed, was greater or less as the flame above approached to or receded from that beneath. There were quite a number of whirlwinds of this class. The fourth kind of whirlwinds were formed of immense columns of smoke, so narrow and lofty that they resembled towers of several hundred feet, or trunks like those of trees in form, extending into the sky. The rotary motion was obvious throughout their entire length. These columns of smoke were generally straight, but sometimes bent at the top by the wind.

In connection with the whirlwinds there were several other facts of interest observed during the burning of the cane. We noticed the direction of the wind was changed. At first it was from the north-east, and continued in that direction in the upper part of the atmosphere, as was evident from the way in which the columns of smoke were bent. But shortly after the commencement of the burning, the air beneath blew in all directions towards the centre of the fire. The columns of smoke were not bent for more than a hundred yards, hence, up to that height, the wind blew in all directions towards the centre.

Secondly, these whirlwinds revolved on their axes from right to left,

and from left to right, without any prevailing tendency to one direction more than to the other. Frequently, the same whirlwind would change the direction in which it revolved, and would again return to its first course. In a few instances this was repeated several times.

Thirdly, in the midst of the fire, the foliage of the trees, which were surrounded by the piles of cane, became gradually dry and parched, and finally caught on fire, burning almost like a flash of powder, and adding greatly to the splendor of the scene. The light red flame of these trees afforded a fine contrast with the deep red flame of the canes, and the nearly white smoke of the burning trunks was also in lively contrast with the heavy, rolling masses of black smoke which surrounded the fire.

Fourthly. The charred leaves of cane, being thin and light, were driven off in considerable quantities. They were carried up, frequently, without being burned, and were sometimes found at a distance from the place of the fire. But, considering the extent of the fire, few cinders were carried up. The combustion was very complete, which I ascribed to two causes:—First, The extremely light and combustible matter of the cane, which would be, for the most part, consumed in the intense heat of the fire, before rising to a great height. Secondly, The air imprisoned in the cells of the cane probably added to the intensity of the combustion. Three conditions—the air confined being in considerable quantity, its position being in the centre of the canes and the centre of the fire, and its high temperature—would contribute to render it more efficient in supporting combustion.

From the great number and variety of whirlwinds which this burning afforded us the opportunity of observing—from the splendor of the flame, and from the beauty of the smoke, rolled in masses above the fire, or in whirlwinds, this scene was one of equal interest to the painter and the student of science. The vast whirlwinds formed in the atmosphere by storms, or by the burnings of forests and dry timber, are either too large and too rapid in their progress to be carefully observed, or they are collected in one column of flame, which has always nearly the same form, and is attended by the same phenomena. But in the burnings of the cane, whirlwinds of a great variety of form and size extend over a wide tract, and all their phenomena are perfectly distinct.

4. On the Apparent Necessity of Revising the Received Systems of Dynamical Meteorology. By W. C. Redfield, New York.

Mr. Redfield said, that, in consequence of the pressure of matter before the Association, he would not now attempt to submit his views on this great subject. He would only say, that his views of the theory of winds had been those which are commonly entertained. But, in pursuing his storm inquiries, he had constantly met with facts and difficulties which appeared irreconcilable with the theory that solar heat is the great or principal cause of the general winds of our globe. To state these objections, in a manner to do justice to his present views of the case, and to explain the principal phenomena of atmospheric motion by the cosmical laws which govern the planetary system, would require more time than could now be allotted. He would, therefore, pass from this topic, and make a few remarks on some recent developments of the law of storms, to which subject his attention had been more immediately directed.

He would here make reference to the Atlantic hurricane in the middle of July, which appeared at Barbadoes and the Windward Islands on the 11th of that month, and, passing thence to the coast of the Carolinas, swept over a large portion of the Atlantic States, in its progress towards the high latitudes. Being preceded by strong southerly winds, which bore large supplies of aqueous vapor, and carrying its axis path over the interior of the country, it was productive of rain in unusual quantities. Its path was more inland, in these States, than most of the storms heretofore traced, which had their course over the same islands.

At the meeting of 1848, in Philadelphia, he had the honor to lay before the Association, the valuable work of Mr. Piddington, of Calcutta, whose labors in illustrating the character and courses of whirlwind storms had, in numerous cases, been the means of saving lives and property from destruction. He would now present to the Association a second work of Col. Reid, "On the Progress of the Development of the Law of Storms," published during the past year in London; a work well calculated for diffusing correct and useful knowledge of this subject.

Mr. Redfield also stated that he had at no time attempted to offer an explanatory theory of the dynamical action in whirlwind storms. He had concerned himself mainly with the facts by which the great law of ROTATION AND PROGRESSION, in determinate directions, was made mani-

fest; but he would not withhold his views of the dynamical theory, could the Association devote its time to this object. He would also allude to another important fact, which should never be overlooked by those who enter on these inquiries. He had found the axis of rotation in the storm-wind to be always coincident with the area of least pressure, as indicated by the barometer, during the entire observed progress of the storm. In thus tracing, geographically, these three essential characteristics of the storm, throughout its progress, we can at once solve the main inquiry, and are better prepared to give due consideration to the aqueous depositions and other diversified phenomena which are contingent to the storm, or to portions thereof, in different latitudes, and at different seasons of the year, as the storm advances from tropical latitudes to the colder regions of the temperate or frigid zones.

Mr. Redfield then laid before the Association copies of a practical rule in seamanship, which is based on the rotary character of storms, and has been printed by Col. Reid, for the benefit of navigators.

RULE IN SEAMANSHIP.

Every rule, applicable to ships which have become involved in storms, is invaluable. Captain Andrews, commander of the royal mail steamer Medway, on leaving Bermuda for England, on the 22d September, 1846, suggested the following:—

By keeping the wind on the starboard quarter, when in a revolving storm in the Northern hemisphere, ships gradually sail from the storm's centre; and by keeping the wind on the port quarter, when in the Southern hemisphere, ships gradually sail from the centre of a revolving storm.

This rule applies to three quarters of the storm's circle. But there is always one quadrant in a progressive whirlwind storm more dangerous than the other three, being that over which the storm's centre passes in its progress; and there would be danger in applying the rule with a ship in this quadrant.

Within the tropics, while the course of storms tends towards the west, the quadrant of greatest danger will be on the west side. But these quadrants will gradually change their position as the storms recurve; and in high latitudes, as the course of the storms becomes easterly, so will these quadrants of greatest danger come to be on the east side of the storm. In order to know which is the quadrant of greatest danger, the theory must be studied until it is understood.

An example of Captain Andrews' rule for the Southern hemisphere

was practised by Captain Moorsom, Royal Navy, when commanding the Andromache frigate at Mauritius, in 1826, and is the more remarkable because it was executed by Captain Moorsom before the theory of storms was understood. Leaving H. M. S. Ariadne, in Port Louis harbor, at the wish of her commander, Captain Moorsom put to sea in the Andromache, with the wind at S. E., veering to S. S. E., predetermined to steer without regard to the compass, and to keep the wind, as it veered, always upon the larboard or port quarter. He had come to this decision from attentively studying the log-books of ships which had encountered hurricanes in the neighborhood of Mauritius, and observed that the wind veered there in a uniform manner. By steering as described, Captain Moorsom gradually carried his frigate away from the centre of the storm, until he had gained its opposite side, and moderate weather. Then, having the wind at N. E., veering to E. N. E, he brought his ship upon the starboard tack, in order to return to Mauritius, which port she reached uninjured.

By comparing extracts from the two log-books, it appears that the Ariadne, in Port Louis Harbor, was in the centre of the hurricane, and had the storm very severely; whilst the Andromache, by putting to sea, and being steered so as to keep the wind on the port quarter, had comparatively moderate weather.

Prof. Henry remarked that the whole subject of storms was one, both in a theoretical and practical sense, which could well engage the attention of the Association. It was one which had been studied in this country more than in any other part of the world; and it was due to those engaged in this research, namely, Mr. Redfield, Mr. Espy, and Dr. Dana, that it should be thoroughly examined by those interested in physics among us.

The subject was one of the most involved, perhaps, of any which could be presented for investigation. Prof. H. had several times resolved to study the subject, but had been deterred from want of opportunity. He thought it would be well to make the subject of the movement of the air, in the case of storms, a special matter of discussion at the next annual meeting of the Association, and that the members of this Section should be requested to direct their attention to the subject during the intervening time. He would prepare himself, and give some views at the next meeting, should he be permitted to attend. He had commenced a series of experiments on the vortices in water, and had arrived at a number of conclusions which he did not anticipate at the commencement. He found three concentric cylinders of motion; one along the

axis, downwards; another exterior to this, upwards; and a third without the two others, downwards.

He had also made a series of experiments on the production of vortices in air. He was inclined to believe that nearly all the phenomena which have been advanced by Mr. Redfield, Mr. Espy, and Dr. Hare, could be illustrated by experiments of this kind.

Prof. Bache remarked that he considered the storms of the United States to be of different classes, and that until those engaged in making the inductions would discuss every observation, (requiring the aid of mechanical computation for such a purpose,) and present the result of each in print, to be examined, no decision likely to stand long could be rendered. It would not do, in discussing a theory, to neglect the contradictions. Prof. B. instanced the paper of Prof. Loomis, on the storms of the United States, presented some years since to the American Philosophical Society, as an admirable specimen of this kind of induction.

Prof. W. B. Rogers stated that three years ago he had made some experiments on vortices in water, by giving rapid rotation to a rectangle of copper plunged a few inches into water contained in a large cylindrical glass jar. The lifting force was very strikingly seen, and at high velocities the descending motion along the axis. He thought that this line of experiments promised very interesting results.

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5. On the Solar Eclipse of July, 1851. By Lieutenant C. H. Davis, Supt. Nautical Almanac.

Having undertaken to prepare for provisional use a set of lunar tables embodying the additions applied by Professor Airy to Damoiseau's tables in order to make the tabular calculations represent as closely as possible Plana's theory, Professor Airy's own corrections from the Greenwich observations, and also the corrections due to Professor Hansen's recent discovery of the two inequalities of long period in the moon's motion arising from the perturbative action of Venus, it is a matter of interest and importance to us to profit by the excellent occasion afforded by the solar eclipse of July, 1851, (visible in the greater part of North America, and the continent of Europe,) to compare the moon's observed place with the tabular place as determined by our tables, and to test the value of the moon's semi-diameter employed in our calculations.

To facilitate and promote the observation of this phenomenon on this continent, I have prepared at the office a chart exhibiting the phases of the eclipse in the United States, Canada, and Mexico, from which, by simple inspection, the observer at any place may find approximately the beginning and end, the duration &c., of the eclipse; and I propose to bind up a copy of this chart with each number of the proceedings of the Association.

This eclipse is remarkably favorable for the approximate determination of longitudes, and cannot fail to be very generally used for this purpose in our country, where such occasions are rendered eminently important by the number of meridians, and the vast extent of imperfectly known territory.

For the elements for general computation, and for the special computations for places in this country, I will refer the observer to the American Almanac, to which the elements and phases of the eclipse have been communicated; and I add here the dates for the principal European observatories, as a means of more accurate and satisfactory geographical determinations.

I have received the permission of the Hon. Wm. A. Graham, Secretary of the Navy, to distribute the chart and accompanying calculations wherever they will be serviceable.

It may be well to say, that in copying the chart upon stone, some of the lines which are crowded in the lower corner of the chart, on the right hand side, have been slightly, (not materially,) distorted.

Dates of Eclipse for Observatories in Europe.

	BEGINNING. h m. s.	END. h. m. s.
Altona,	2 49 51·6	4 56 34·0
Berlin,	3 8 18·0	5 12 58·6
Brussels,	2 27 6·0	4 36 33·7
Cambridge,	2 3 18·8	4 15 9·0
Danzig,	3 30 4.7	5 31 35·7
Dorpat,	4 0 19·0	5 57 0·4
Göttingen,	2 53 20·9	4 59 54·2
Edinburgh,	1 43 57·2	3 55 52·4
Greenwich,	2 04 09·5	4 15 50·6
Königsburg,	3 37 58·4	5 38 29·8
Liverpool,	1 57 37·1	4 11 00·4
Nicolajew,	4 43 00·3	6 35 47·0

	h. m. s.	h. m. s.
Paris, ...	2 20 38·8	4 30 28·9
Pulkowa, ...	4 14 8·0	6 8 32.2
Regent's Park, . .	2 03 24·1	4 15 8·7
Rome, ...	3 24 04·6	5 24 55·1
Vienna, ...	3 30 12·3	5 32 19·7
Warsaw, ...	3 44 42·9	5 44 50·5
	Total Phases.	
Danzig	4 31 28·4	4 34 35·1
Königsberg, . . .	4 38 48·9	4 41 43·4
Nicolajew, ...	5 40 18·8	5 42 29·3
Warsaw, ...	4 46 1·0	4 48 51·5

6. On the Numerical Computation of the Co-efficients of the Perturbatory Functions of Planetary Motion. By Sears C. Walker, Washington.

[*Not received.*]

7. On the Diminution of Temperature in Air by Expansion, and the Permeation of Air by Moisture. By Prof. J. P. Espy, Washington.

A very extended series of experiments with the double nephelescope was made last winter and spring, in the Smithsonian Institution, by me, on the degree of cold produced in air, both dry and moist, by sudden expansion from diminished pressure. The expansion was effected by opening a communication between two vessels containing air of different known densities at the same temperature. This communication was cut off at the moment when equilibrium of pressure was effected between the two vessels, and the degree of cold was calculated from the increase of elastic force which the expanded air acquired in regaining its original temperature. When moist air was used, an allowance was made for the elastic force of the vapor generated during the rise of temperature. This additional force was calculated on the supposition that the dew-point rose with the temperature, and was the same as the temperature of the air in the vessel, both before and after the expansion. This, however, was found not to be the case, after the expansion, though it was before by compulsion. This fact and the following are some of the results which have been obtained from a patient analysis of these nu-

merous experiments. When dry air at common pressure was expanded one-third, that is, when one-third was allowed to escape, at the temperature of 52° Fahrenheit, it was cooled about 47°, when it was expanded ½, about 72°, when it was expanded ⅔, it was cooled about 100°, and according to this ratio, if it had been expanded into a vacuum, it would have cooled about 162°, and of course would then be 110° below zero.

When the air was above 52°, it was cooled one per cent. more for every five degrees above, and it was cooled one per cent. less for every five degrees below 52°.

This corresponds nearly with what is known to be the normal temperature of the atmosphere when clear, at great heights, and therefore in the torrid zone, when the temperature below is about 82°, the temperature of the top of the atmosphere is about 89°·44 below zero.

In the frigid zone, where the temperature is about 2° below zero, the top of the atmosphere is about 123°·52 below zero.

When condensed air was permitted to expand into rarified, a greater cold was produced in air of greater density for a given expansion.

A double density gave an increase of cold of 15½ per cent., 2⅔ density gave an increase of cold of 20⅓ per cent., and 3⅓ density gave an increase of cold of 26 per cent.

Moist Air supposed to be Saturated.

When moist air supposed to be saturated was used, it was cooled less for a given expansion by a little more than one degree for each one hundredth of an inch of elastic force of vapor condensed by the cold of expansion.

The specific caloric of air came out from these experiments about 0·1800.

Suspecting, from this result and from other circumstances, that the air was not charged up with vapor at the end of the experiment to the temperature of the air itself, I varied the experiment, so as to know the actual dew-point both before and after the expansion, by using common air whose dew-point was carefully ascertained, and no water was put in the vessel, as there was in the former case. When this was done, the specific caloric of air at half common density came out about 0·2839, and at common density about 0·2188, and at 1¼ density about 0·1690, and for each one hundredth of an inch of elastic force of vapor condensed, common air was hindered from cooling, for a given expansion, about one degree.

When air saturated with moisture at ¼ common density is expanded

into air $\frac{1}{8}$ the density, it cools enough to produce a very faint cloud, but when saturated air of $\frac{1}{5}$ common density is expanded into air $\frac{1}{10}$ common density, the cold of expansion is not sufficient to make any visible cloud. Therefore, when an ascending column of air rises to where the barometer stands only 6 inches high, if it has not condensed all its vapor in going to that height, it will cease to condense at that point, and go to the top of the atmosphere, clear.

If the elastic force of the vapor at the surface of the earth under a storm-cloud is 0·600 of an inch, it will be all condensed in rising to where the barometer stands six inches.

If the elastic force of the vapor at the surface of the earth is 0·800 of an inch, as it is in the torrid zone, then will there remain in the air, after it ascends in a storm-cloud to the top of the atmosphere, a quantity of vapor, which, however thin there, would have an elastic force of 0·200 of an inch, if the air containing it should be condensed in common density at the surface of the earth.

This is about the quantity carried over by the upper trade winds from the torrid zone to higher latitudes; a much less quantity than is carried by the trade winds below into the torrid zone.

This conclusion is made on the supposition that aqueous vapor might be indefinitely expanded with the air as it ascends, without losing its elasticity, which is hardly probable; though its deviation from this law will, it is believed, not change much the above result.

When the elastic force of the vapor in the air is 0·600 inches and above, the ultimate difference of temperature between the ascending column of air in a storm-cloud, and that of the air on the outside of the storm-cloud, at the same height, is 60° Fahrenheit; and when the elastic force of vapor at the surface of the earth is less than 0·600 of an inch, the vapor all condenses into cloud before the ascending column reaches to where the barometer stands six inches high, and the temperature of the ascending column will be from the point where all the vapor is condensed to the top of the atmosphere, about one degree higher in the ascending column than on the outside at the same height, for each hundredth of an inch of elastic force of the vapor at the surface of the earth.

It follows that the top of all storm-clouds is flat, and that they are more and more lofty as the dew-point is higher and higher, until the elastic force of the vapor at the surface of the earth is 0·600 of an inch, and then the top is where the barometer stands at about six inches, beyond which the top of the cloud does not rise, however high the dew-point may be.

The top of the cloud, when fully formed, will not only be flat, but much wider than the base, in consequence of the outspreading of the ascending column of air at this great height. If any one observes this extended rim when properly situated for observation, he will discover that it turns to the thin hairy cloud called cirrus, and that the particles of it, being removed from over the upmoving current, gradually subside by their specific gravity through the air, in which they slowly evaporate.

Vapor permeates air from a high to a low dew-point, with such extreme slowness, if it permeates it at all, that in meteorology, it may be safely said, it rises into the upper regions only with upmoving currents of air.

If vapor permeated air, as is generally supposed, the torrid zone over the sea would be covered with eternal cloud.

Air in contact with water will not saturate itself, or, if saturated at first by force, will gradually lower the dew-point below the temperature of the air to the amount of $4\frac{1}{2}°$ in 15 days.

If air descends from the upper regions in temperate latitudes, it will be able to contain, on account of the heat of condensation, from six to ten times as much vapor below as it contained above.

The upmoving current in storms, and the fall of the barometer, are accounted for by the evolution of latent caloric alone in the formation of cloud.

The temperature of space above the atmosphere is some degrees, though not very many, colder than the top of the atmosphere over the region of greatest cold near the poles, in the coldest season of the year.

If the greatest cold at the surface of the earth is 60° below zero, then the temperature at the top of the atmosphere would be colder by 128°·40, and would therefore be 188°·40 below zero. The temperature of interplanetary space is some degrees lower than this lowest temperature.

The method of deriving these results and many others, from the experiments, will be given in my third report on meteorology, to the Secretary of the Navy.

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8. On the Distribution of the Orbits of the Asteroids. By Dr. B. A. Gould, Cambridge.

[*Not received.*]

9. On Barometrical Admeasurements, and the Distance to which Corresponding Observations may be used for that Purpose. By Prof. Arnold Guyot, Cambridge.

[*Not received.*]

After the close of this communication, Mr. W. C. Redfield made some remarks upon the plan of barometric operations used by him many years ago, for determining heights in the northern part of the State of New York. He employed several stations or bases for comparison, viz.—New York City, Albany and Syracuse, and in favorable weather he found his results to correspond in a satisfactory degree.

Mr. Boutelle described the mode of making barometric observations in the boundary line survey between the United States and British Provinces. These operations proved that single observations made at distant points cannot, in general, be depended on, even for approximate results, and that to secure much accuracy, the mean must be taken of a series continued for a long time.

10. Method of ascertaining the Velocity of the Galvanic Current. By Orange Judd, Yale Analytical Laboratory.

[*Not received.*]

The four following papers were, in the name of the authors, presented to the Association by Prof. Joseph Henry :—

11. Account of a new Air-Pump, invented by L. P. Hardeland.

[*Not received.*]

12. Suggestions as to the Consruction of an Electro-Magnetic Automaton. By C. Dowden.

[*Not received.*]

13. Proposed Experiments on the Cohesion of Liquids. By Lieut. C. B. Hunt, U. S. Navy.

[*Not received.*]

14. A new Method of finding the Number of Vibrations corresponding to the Sounds of the Natural Scale, by dividing the Vibrations of the Fundamental Sound by the Numbers Eight and Nine. By H. Hirzel, of Lausanne, Switzerland.

If we express by x a number whatever of vibrations, and by 24 the number of vibrations of a determinate sound, we have

$$8 : 42 = 3 \qquad\qquad 24 : 9 = 2\tfrac{2}{3}$$

$$x : 8 = \frac{x}{8}, \qquad \text{and} \qquad x : 9 = \frac{x}{9},$$

		A. Vibrations.		B. Vibrations.		
Quotient. Constant Factor. 3×1	Do	24	. . . . . Do . . .	24	Do	Constant Factor. Quotient. 3 × 2⅔
	Re	27	. . . . . Re	26⅔		
	Mi	30	. . . . . Mi	29⅓		
3×2		33	Fa . . .	32	Fa	
	Sol	36	. . . . . Sol	34⅔		3 × 2⅔
3×3		39		37⅓		
		42	. . .	40	La	
	Si	45	. . . . . . Si			

. . . .

In the column of A we find four sounds of the natural scale, namely the 2d, the 3d, the 5th and the 7th, or Re, Mi, Sol, Si; and two in the column of B, namely, the 4th and the 6th, or Fa and La.

A. The vibrations corresponding to the sounds of the column of A, (Re, Mi, Sol, Si,) are, according their mode of progression, liable to be divided into three distinct parts:—

1. That of tierce, including the vibrations of Re and Mi. Here we find the number of vibrations by adding the number 3 (quotient of 24 by 8) to

the number of vibrations of Do and Re. The vibrations of Do being 24, we have : $24 + 3 = 27 =$ Re ; $27 + 3 = 30 =$ Mi. The progression is arithmetical.

2. Second part, or Sol. Here the progression of the vibrations from Mi to Sol is equal to twice the quotient $2 . \frac{x}{8}$. For instance, the vibrations of Mi being 30, those of Sol must be 36.

3. Third part, or Si. The progression from Sol to Si is equal to three times the quotient of 24 by 8 $\left(3 \times \frac{x}{8}\right)$. The vibrations of Sol being 36, those of Si must be 45.

B. We met within the column of B only two sounds of the natural scale, namely the 4th and the 6th, or Fa and La, for which the corresponding number of vibrations are not found among those of the first column. The progression of vibrations from Do to Fa is the same as that from Fa to La, namely, the time the quotient of the vibrations of the fundamental sound divided by 9 $\left(3 : \frac{x}{9}\right)$. The vibrations of Do being 24, those of Fa must be $24 + 3 \cdot 2\frac{2}{3}$, or 32 $\left(x + 3 . \frac{x}{9}\right)$; and those of Fa being 32, the number corresponding to La will be $32 + 3 \cdot 2\frac{2}{3}$, or 40 $\left(x + 6 . \frac{x}{9}\right)$.

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15. Analysis of the Dynamic Phenomena of the Leyden Jar. By Prof. Joseph Henry.

Professor Henry gave an account of his investigations of the discharge of a Leyden jar. This was a part of a series of experiments he had made a few years ago, on the general subject of the dynamic phenomena of ordinary or frictional electricity. On this subject he had made several thousand experiments. He had never published these in full, but had given brief notices of some of them in the proceedings of the American Philosophical Society. All the complex phenomena he had observed could be referred to a series of oscillations in the discharge of the jar. If we adopt the hypotheses of a single fluid, then we shall be obliged to admit that the equilibrium of the fluid, after a discharge takes place, by a series of oscillations, gradually diminishes in intensity and magnitude. He had been enabled to show effects from five of

these waves in succession. The means used for determining the existence of these waves was that of the magnetization of steel needles, introduced into the axis of a spiral. A needle of this kind, it is well known, is susceptible of receiving a definite amount of magnetism, which is called its saturation. Now, if the needle be of such a size as to be magnetized to saturation by the principal discharge, it will come out of the spiral magnetized to a less degree than that of saturation, by the amount of the adverse influence of the oscillations in the opposite direction to that of the principal discharge. If the quantity of electricity be increased, the power of the second wave may be so exalted that the needle will exhibit no magnetism; the whole effect of the first or principal wave will be neutralized by the action of the second. If the quantity of electricity be greater than this, then the needle will be magnetized in an opposite direction. If the electricity be still more increased, the needle will again exhibit a change in its polarity, and so on in succession, as the power of the successive waves is increased.

These experiments had been made several years ago, but Prof. H. had not given them in detail to the public, because he had wished to render them more perfect. For the last three and a half years, all his time and all his thoughts had been given to the details of the business of the Smithsonian Institution. He had been obliged to withdraw himself entirely from scientific research, but he hoped that now the institution had got under way, and the Regents had allowed him some able assistants, that he would be allowed, in part, at least, to return to his first love—the investigation of the phenomena of nature.

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16. Description of a New Instrument for Measuring the Angle contained between the Optic Axes of Crystals, and for Goniometrical Purposes. Accompanied by the Angles contained between the Optic Axes of some American Micas. By W. P. Blake, New Haven.

On commencing the optical examination of the mineralogical species Mica, with the intention of ascertaining the relations of the angle contained between the axes of *no* double refraction and their other physical properties, I was unable to proceed for want of an instrument combining all the requisites for the examination of that peculiar class of minerals. I therefore designed and constructed one peculiar in the

form and arrangement of the several parts. Its distinguishing peculiarities are:—

First. The arrangement of the several parts so as to allow of great variation in the size and weight of specimens to be examined.

Second. The *vice*, or *clamp*, for firmly securing a specimen in the axis of the instrument.

Third. An arrangement of lenses combined with the analyzing piece, by which the wide and extended circles or ellipses surrounding the poles of the optic axes in *thin* plates are condensed and brought together, so that their form and arrangement may be seen without difficulty, and permitting more accurate measurements to be obtained.

It is intended by this construction and general arrangement of the parts, to provide for the mineralogist an instrument combining all the requisites for different mineralogical angular measurements.

The graduated circle is supported in a vertical plane as in the goniometer of Dr. Wollaston, and, like it, has a hollow axis, in which the horizontal shaft for the support of the mineral to be examined turns, its axis of rotation being at right angles to the plane of the circle, and in its centre.

The light is polarized by reflection from a black mirror attached to the base of the instrument, and capable of being inclined at such an angle as will insure the polarization of the light and its reflection across the end of the horizontal shaft, and through the axis of the eye-tube (above, containing the analyzing piece, lenses, &c.) emerging to the eye at a convenient angle for a seated observer.

For securing the flat plate or film of mineral to the shaft, I have provided two stout jaws, or projections, on the end of it, and on each side, and parallel to the axis of the shaft, each projection having a screw working in it towards the axis. The ends of the screws are tipped with movable cheeks, between which plates of different thickness may be firmly secured, and brought completely into the axis of the instrument with accuracy and ease: the form of this clamp also allows of the plate being moved in its own plane, in order properly to adjust the poles of the optic axes with respect to fixed lines.

For the purpose of reducing the size of the colored rings about the poles in *thin* plates, I bring next to the mica, and between it and the analyzing piece, one, two, or three short focus convex lenses, placed near together, so that their combined effect will be to render the rays that pass through the tourmaline analyzing piece to the eye, as diverg-

ing as is necessary to produce the desired reduction of the rings. I have also made use of a concave lens to produce a similar effect.

To adjust the instrument for obtaining the angle with common solar light, it is placed near an open window, so as to receive upon the mirror the light reflected from the clouds. Two strings are to be stretched across the open window space—one vertically, the other horizontally—intersecting at right angles. The instrument is then so placed that the point of intersection of the images of these strings shall be in the centre of the field of view. Care must also be taken that the axis of the eye-tube prolonged to the image of the string, shall cut the axis of rotation of the mineral. The analyzing piece may be either a Nichols prism or a tourmaline, and should be turned so as to extinguish the ray, before commencing the adjustment of the mineral.

To obtain the angle contained between the optic axes of a plate or lamina of mica, it is to be placed between the cheeks of the clamping screws, securing it in such a position that by inclining the plate to the path of the polarized ray the elliptical rings surrounding the poles are successively brought into the field, and bisected in the direction of their longer axis by the vertical line. Zero on the circle and vernier then being made to coincide, the shaft carrying the mineral is gently turned until the system of rings is bisected by the *horizontal* line. The arm carrying the vernier is then clamped to the shaft, and both are turned together until the rings of the other pole arrive at the same position with respect to the fixed lines. The angle through which the mica has moved is then read off upon the circle.

The measurement of the angles of the Muscovite species of mica minerals, where the angles are mostly above 60°, is unattended with difficulty; but for the species "Phlogopite," (angles always below 20°) the angles are often very small, and it becomes necessary to have plates of considerable thickness, in order to observe their *biaxial* character and distinguish the poles; for with plates of the thickness of those used for the measurement of the angle of the Muscovite species, but *one* system of rings can be seen, and these are so nearly circular and simulate the characteristics of the cross and rings of the uniaxial crystals, that they are with difficulty distinguished from them.

It is most probable that several dark colored micas, now considered uniaxial, would give decided proof of their biaxial character, if it were possible to examine them in thick plates.

So much time was occupied in the construction of my instrument,

that I am not able, at this meeting, to present as many measurements as I desired. I have obtained the following as the angles for specimens from the localities named. Clear, transparent specimens, the laminæ of which had not been separated by cleavage, were selected. No allowance is made in these results for the errors of refraction which are slight in mica; but these slight errors, together with the deviation of the rays in consequence of the polarization by laminæ, indicated by M. Biot, will receive more attention in future measurements.

Locality.	Color, &c.	Angle between optic axes.
Alstead, N. Hampshire,	Brown, in large plates, with quartz and tourmalines, . .	68° 58′
Do. another species	Do. do.	68° 30′
Acworth, N. Hamp. .	Brown, in large, clear plates, .	72° 15′
Grafton, " .	Do. do.	69° 33′
Orange, " .	Yellowish green, large irregular plates, with compressed brown tourma-lines, . . .	74° 30′
Paris, Me., . .	Apple green, clear, with small radiated green tourmalines, .	72° 15′
Do. " . .	Brown in thick plates, clear,	72° 50′
Monroe, Ct., . .	Brown, shade of yellow, large plates,	70° 30′
Haddam, " . .	Pale yellow, with compressed garnets, ...	71° 14′ to 71° 30′
Royalston, Mass., .	Transparent, clear, . .	70° 20′
Philadelphia, near Fairmount, Pa., .	Do. do. . .	63° 15′
Pennsbury, Pa., .	Pale gray, with markings, .	59° 30′
Do. another species	Do. do. . .	61°
Tucker's Quarry, near Wilmington, Del.,	Brown, ...	67° 70′

In addition to the specimens I was able to collect from localities and cabinets, Prof. B. Silliman, jr. supplied me with many, and has kindly allowed me the use of his specimens.

It is my purpose to continue the investigation with more chromatic lights, and to ascertain the thermal relations of the species.

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17. On the Extension of Bode's Law. By Prof. Stephen Alexander, Princeton.

Prof. Alexander showed how, by a modification of Bode's Law, the distances of the planets from the sun, and also the distances of the satellites of Jupiter, Saturn, and Uranus, from their primaries, are well represented.

18. In conclusion Mr. G. P. Bond exhibited a machine for continuous uniform motion, the joint invention of Mr. W. C. Bond, of the Cambridge Observatory, and of his sons, G. P. and R. F. Bond.

Its principle is the use of elasticity in such a manner as to allow of the introduction of the dead beat or other escapements and of the common pendulum in machinery where the motion of the trains of wheels is continuous. In the apparatus exhibited the circumference of the escapement wheel, which was regulated by a half-second pendulum, was separated from the rest of the train by a steel spring.

In attaining the objects proposed, the Messrs. Bond have been entirely successful.

The Section then proceeded, according to the appointment of the previous day, to the discussion of the question of Galvanic Wave time.

Dr. Gould inquired of Mr. Culmann, in what manner he decided whether the wave passed through the wire or through the earth.

Mr. Culmann explained his method. He then cited a fact by which he considered it to be proved that in some cases the wave must pass through the ground. This was the observation of Mr. Morse, many years ago, that when two parallel wires on opposite sides of a river are connected with the water on those sides, the signals made on one side will always be repeated on the other.

Dr. Gould said that it must first be proved that the current moves less rapidly through the wire than through the ground. He could not, from the experiments on the night of the 24th, find any indication of the currents having passed through the ground.

Prof. Loomis remarked on the improbability that the velocity of the electric wave through the earth was less than that through a small wire, and also spoke of the necessity of a continuous circuit in order to produce magnetic effects by means of the electric current.

Prof. Hare said that he did not view the earth as comparable with the wire conductor. It seemed to him rather a great reservoir. He regarded galvanic electricity as a polarization of the ether associated with ponderable matter. There was a great difference between the conduction of frictional electricity and galvanic electricity, the one being proportional to the surface of the wire, and the other to its sectional area.

Mr. S. C. Walker remarked upon these results, and said that although he admitted the resistance of the earth to be less than that of a wire, it did not hence follow that the velocity of the fluid through the earth must be the greater.

SIXTH DAY, SATURDAY, AUGUST, 24, 1850.

SECTION OF CHEMISTRY AND MINERALOGY.

The Section met at 10 o'clock, A. M., in its usual place. Prof. SILLIMAN, Jr., was called to the chair, Dr. R. E. ROGERS, Secretary.

The following communications were presented.

1. ANALYSIS OF PHLOGOPITE MICAS FROM ST. LAWRENCE CO., NEW YORK. By WM. J. CRAW, Yale Analytical Laboratory.

[*Not received.*]

At its conclusion, Dr. GIBBS inquired of Mr. Craw by what method he had determined the fluorine, to which the latter replied, by the process prescribed by Rose and Berzelius, as fluoride of calcium.

Dr. GIBBS expressed his opinion of the uncertainty of all the processes, and stated that though he had not tried the one above referred to, he concluded that it was not reliable, from the caution with which it was recommended by Rose.

In reference to Mr. Craw's paper on the analysis of phlogopite micas, Prof. SILLIMAN, Jr., remarked that this research possessed much interest as confirming the results of his optical examinations of the American micas. It appears that the wide difference in the value of the optic axes of the phlogopites from the muscovites is sustained by an equally remarkable difference in their chemical constitution. The muscovite and biotite species are apparently confined in great measure to the granitic rocks, while the phlogopites and other binaxial micas of a very low angle between the optic axes, occur almost without exception, in calcareous deposits. Many of the so called uniaxial micas (biotites) which have been described as coming from calcareous deposits, Prof. S. had, in his recent researches, satisfied himself, belong to the species phlogopite. Further and multiplied analyses of the dark colored micas from the lime rocks of New Jersey and New York are essential to the correct understanding of this subject; and in this view Mr. Craw's paper must be regarded as one of much interest.

Dr. GIBBS expressed the opinion, which he believed was not given in the books on the subject, that we could not conclude from the isomor-

phism of two compounds, that the angles between the optical axes are equal, giving, as an instance, sulphate of iron and sulphate of magnesia. He believed there must be something not yet known, a *tertium quid*, which regulates the distribution of the ether in the interior of a crystal to account for all the facts observed.

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2. Artificial Minerals from an Iron Furnace, in Easton, Pa. By Dr. C. T. Jackson.

During this summer I received from Mr. Charles Jackson, Jr., of Boston, a number of beautifully crystallized slags from the furnace at Easton, Pa., some of the crystals being in the slags, and the others in a salamander of iron. I exhibit these specimens, as they are of high geological, mineralogical, and chemical interest, serving to illustrate the igneous origin of crystallized minerals. Those in the slags are opaque, and have a drab color, like that of the massive slag. The crystals are perfectly formed, and are cubes or right square prisms in some of the specimens, while in others the form of the six-sided prism appears, and in others, the crystallization has assumed the botryoidal form. I exhibit, also, specimens of crystals, resembling in appearance some varieties of sulphate of baryta, which consist of crossing laminæ of great delicacy, and are transparent in some of the specimens. These I heav only examined qualitatively, and find them to consist of silica, lime alumina, and oxide of iron, the two first ingredients greatly predominating. Another specimen I present has obtuse rhombohedral crystals, which are glassy, and the whole slag is vitreous.

The other specimens are from the salamander, a mass of reduced iron and slag, that slowly cooled in the furnace. These specimens are of the highest mineralogical interest, for they contain perfectly defined transparent crystals of known igneous origin. Some of them resemble so closely the appearance of chabazite, as to be readily mistaken for that mineral. Others have the clove-brown color of axinite, and are more acute rhombohedrons than those previously noticed. This specimen marked A, I have examined, and have obtained the following results. Description—form rhombohedral, with edges replaced by narrow planes. Color, clove-brown, lustre, adamantine, hardness 5.5. Sp. gr. not discovered—transparent—anhydrous.

Qualitative chemical examination proved that the mineral consisted of

silica, lime, oxide of iron, oxide manganese, and a little alumina. Proportional analysis of 1 gramme gave per cent.

Silica,	33·70
Lime,	31·80
Peroxide of iron,	18·00
Manganeso-Manganic oxide,	14·90
Alumina,	3·50
	101·90

Since the iron and manganese were in a lower state of oxidation than is given in the analytic results, the mineral will consist of

$\dddot{\mathrm{Si}}$.	33·70 containing ox. =	17·49	
$\dot{\mathrm{Ca}}$.	31·80	9·04	17.76
$\dot{\mathrm{Fe}}$.	16·20	3·99	
$\dot{\mathrm{Mn}}$.	13·96	3·10	
$\ddot{\mathrm{Al}}$.	3·50	1·63	
	99·16		

It is possible that part of the iron and manganese may exist in a higher state of oxidation than is given in this analysis, but the color of the mineral and the presence of metallic iron seems to indicate that they must be in the state of protoxides. The iron made in the furnace is quite strongly charged with manganese, and is suitable for making bar iron, for which it is chiefly used.

While making this communication, Dr. J. exhibited a number and variety of crystalline products, some of which resemble chabazite, sulphate of baryta, &c., in appearance.

In this connection he also exhibited artificial sulphuret of zinc and copper from other furnaces, which resemble hornblende. Specimens of native copper from Lake Superior were likewise shown to the Section.

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3. Zircon, Sodalite, Cancrinite, &c., from Litchfield, Me. By Dr. C. T. Jackson.

[*Not received.*]

Among the specimens, an uncommonly large one of zircon was shown. Dr. Gibbs stated that he had analyzed this same variety of zircon,

and had reason to believe that it consisted of an earth different from zirconia.

Prof. SILLIMAN Jr., inquired of Dr. G. if he had extended his examination to other variety of zircons, to which Dr. G. replied that he had not, but regarded it an important line of investigation for the purpose of comparison.

Dr. JACKSON concluded his notices of minerals in exhibiting a specimen of native copper which was coated with a substance which Mr. Teschemacher and Dr. Hayes had pronounced to be a compound of vanadium, and in detailing some analyses of cast irons which contained extraordinary quantities of manganese.

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4. DETERMINATIONS OF NITROGEN IN TWO VARIETIES OF INDIAN CORN. By WM. H. BREWER, Yale Analytical Laboratory.

The first variety selected for this examination is known commonly as "large sweet corn." The entire kernel was used for analysis, dried at 110° C, or 230° Fahr. in a current of dry air, and the nitrogen determined as ammonia, by combustion, according to Varrentrapp and Will's method.

All the per centages are given on the material dried as stated. The average results of six combustions of this variety gave as the mean, 1·839 per cent. nitrogen. This is sufficient for the existence in the grain of 11·488 per cent. of gluten, protein, or other similar nitrogenous compound, allowing it to contain 16 per cent. nitrogen.

The respective results of the six combustions were as follows:

	Nitrogen.		Gluten or Protein.
	1·818	equivalent to	11·362
	1·775	"	11·094
	1.811	"	11·319
	1.844	"	11·525
	1.823	"	11·393
	1·945	"	12·156
Giving the mean	1·839	"	11·488

The next variety examined, is the most common "eight rowed yellow" that is cultivated in this vicinity. The mean of two combustions

gave 1·9704 of nitrogen, equivalent to 12·314 of gluten or other nitrogenous compound. The two combustions gave, respectively,

	Nitrogen.		Gluten.
	2·0094	equivalent to	12·559
	1·9310	"	12·069
Producing the mean	1.9704		12.314

These results seem to indicate that sweet corn contains *less* gluten than hard yellow, instead of more, as has lately been stated, and also that both of these varieties are more nutritious as food for either man or his animals, than this grain has been represented by some chemists. I think that additional investigations, carefully pursued, will further establish this view, which, judging from the practical use of this grain, has been long entertained by the agriculturists of this country.

I will mention in this connection, that the cobs of these same varieties were examined, and the nitrogen they contained determined, by Mr. M. C. Weld, of the Yale Laboratory, principally to determine their economic value for feeding stock when ground with the grain.

The material was dried in the same manner as before stated, only at a higher temperature, he employing the temperature of 120° C. or 248° Fahr., and the per centages are given on the thus dried material.

The mean of several combustions gives as the result, the per centage.

	Nitrogen.		Gluten, protein, or other similar compounds.
Sweet corn cobs,	·453	equivalent to	2.831
Yellow " "	·305	"	1·906

This shows that the nitrogenous substances of the cob are sufficient to give it a considerable value as a nutritive article of food, aside from the other organic compounds of value it may contain, as the nitrogenous compounds are present in as large or even larger quantity than the potato is said to contain.

Analysis of the Inorganic Constituents of a variety of Indian Corn.

The variety selected for this examination is one of the same that was used in the investigations, the results of which were just read,—viz., the "large sweet corn." As usually found in the open air, at the ordinary temperature and moisture, it contained from 11 to 11·5 per cent. of water which could be driven off at 212° Fahr. When thus dried, two specimens yielded, after burning, respectively, 2·05 and 2·08 per cent. of ash.

The composition of this ash, (which it is to be noticed is of the whole kernel, including the bran and the heart or germ) is as follows:

Potash,	26·031
Soda,	13·067
Chlorid of sodium,	·293
Lime,	1.178
Magnesia,	13·281
Peroxyd of iron	·879
Phosphoric acid	44·603
Sulphuric acid	trace
Silicic acid	3·91
	99·723

It will be perceived that, like other varieties of this grain, the ash consists almost entirely of the phosphates of the alkalies and alkaline earths, the former predominating, and the potash being present in far larger proportion than soda, and magnesia far larger than lime. The sulphuric and silicic acids were present in smaller proportions than is common, only traces of the former being found.

The phosphoric acid was separated from the bases for determination, according to Rose's process, by means of metallic mercury and nitric acid, forming a nitrate and phosphate of the suboxyd of mercury, and nitrates of the bases, and separating the phos. acid from the mercurial salt by fusion with alkaline carbonate and precipitation as phos. of magnesia. The remaining constituents were determined in the usual way, the alkalies separated from each other by means of bichlorid of platinum.

These investigations were pursued at the suggestion of Prof. J. P. Norton.

Dr. Jackson inquired of Mr. Brewer, whether he had analyzed the different parts of these varieties of corn, expressing his conviction of the great importance of such an investigation, and stating that he had found all the phosphoric acid in the chit. He here communicated the results of his examination into the quantity of oil contained in the varieties of corn and the parts of the grain, and of his inquiries as to the kind best suited for parching, and the increase in bulk during this process, which amounted to thirty fold.

Mr. Brewer, in reply, stated that his analyses had extended so far into

the different parts of the grain as to enable him to ascertain that the phosphates did not reside only in the chits, but were contained throughout the whole grain.

Prof. SILLIMAN, Jr., inquired of Mr. B. whether he had detected in the ashes of the corn any organic salts.

To this question, Mr. B. replied in the negative, stating that he had tested for organic acids, by the treatment of an aqueous solution of the ash, with nitrate of silver, as described in a report of the New York Agricultural Society.

Prof. NORTON remarked that these results by Mr. Brewer possess some interest to the Section, as they show that those obtained in the proximate analyses of grains need confirmation by ultimate analyses. Sweet corn has been stated by Prof. Emmons and Mr. Salisbury to contain about 24 per cent. of nitrogenous substances, but is here shown to contain in the present instance less than half that amount. He considered that the results of proximate analyses of this kind were not generally to be depended upon, unless confirmed by ultimate analysis. This was particularly applicable to the nitrogenous bodies the proximate determinations of which were extremely uncertain, Prof. Norton further added, in confirmation of Mr. Brewer's statement, that he had in no case found organic salts in the ashes in the many analyses he had performed.

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5. ON THE PRESENCE OF MALATE OF LIME IN THE SAP OF THE ACER SACCHARINUM. By T. S. HUNT.

[*Not received.*]

The process for obtaining malic acid from this source was described by Mr. HUNT.

Dr. R. E. ROGERS remarked, that if it should become desirable to procure malic acid in quantity, he thought the source pointed out by Prof. Wm. B. Rogers, in an article communicated many years ago to the American Journal, would be found both convenient and very productive, viz: the Rhus glabra, since its nearly ripe berries, in the fall, become coated with a thick, abundant efflorescence of almost pure bimalate of lime, which is readily soluble in water.

SEVENTH DAY, MONDAY, AUGUST 26, 1850.

MEETING OF THE STANDING COMMITTEE.

The Standing Committee met at 8½ A. M., for the purpose of completing the unfinished business of the American Association, authority for this purpose having been delegated to them by a vote of the Association.

Prof. A. D. BACHE submitted a series of rules of organization, which were referred to a committee consisting of Profs. BACHE, HENRY, and BAIRD, with power to alter and adopt them in the name of the Standing Committee.

The following resolutions were then adopted :—

Resolved, That the Treasurer be requested to retain three hundred dollars of the funds in his hands, and belonging to the Association, for the purpose of paying the salary of the Permanent Secretary,—said payment to be made at such time and in such manner as may be agreed upon by the Treasurer and Permanent Secretary.

Resolved, That the thanks of the American Association be presented to the Treasurer of the Association, Dr. ALFRED L. ELWYN, of Philadelphia, for the great assiduity and faithfulness with which he has discharged the duties of his office.

Resolved, That Prof. HENRY be requested to address the Association at its meeting in Cincinnati in May next, on the present condition of American science, and the means for its advancement.

Resolved, That copies of the proceedings of the American Association be presented to the New York Lyceum, and to the Philadelphia Academy of Natural Sciences.

Resolved, That Profs. OLMSTED and BAIRD be a committee to make arrangements for the reporting of the proceedings of the next annual meeting at Albany.

Resolved, That the subject of reporting the proceedings at the Cincinnati meeting in May next, be referred to the Local Committee of said meeting.

Resolved, That the Permanent Secretary be requested to provide minute books, suitably ruled for the list of members and titles of papers, minutes of the General and Sectional meetings, and for the other purposes indicated in the rules.

Resolved, That the Permanent Secretary be requested to inform the authors of communications, that abstracts, or entire papers, must be furnished before the 1st of December next, and that otherwise they cannot appear in the forthcoming volume of proceedings.

Resolved, That Profs. BACHE, HENRY, and BAIRD, be a sub-committee to prepare and print, in the form of abstracts, the rules approved at the present meeting of the Standing Committee, and which are hereby provisionally adopted for the next meeting of the Association.

Resolved, That the Permanent Secretary be requested to report to the Standing Committee, at the next meeting of the Association, a list of colleges, of which the Professors in branches of science are members of the Association, with the names of the Professors; and a similar list of those who have not yet joined the Association.

Resolved, That a committee be appointed to consider the propriety of memorializing Congress on the subject of authorizing the addition of a corps of naturalists and geologists to the Mexican Boundary Survey; said committee to have power to act on behalf of the Association.

Also Resolved, That this committee consist of Prof. AGASSIZ, Prof. SILLIMAN, Dr. S. G. MORTON, Prof. WM. B. ROGERS, and Prof. WOLCOTT GIBBS.

Resolved, That Prof. B. SILLIMAN, jun., be added to the Local Committee for the Cincinnati meeting.

Resolved, That whenever the Permanent Secretary notices any error of fact, or unnecessary repetition, or any other important defect in the papers communicated for publication in the proceedings of the Association, that he be authorized to commit the same to the author, or to the proper sub-committee of the Standing Committee, for correction.

Resolved, That Prof. HENRY be a sub-committee to draw up a system of scientific ethics, to be submitted to the members of the Association.

Resolved, That it be recommended to the Association, at its next annual meeting, that the annual contribution be raised to two dollars,

and that the article of the constitution referring to this subject be altered accordingly.

At the request of Prof. W. R. Johnson, it was

Resolved, That a committee be appointed to examine the experiments on coal by Prof. Johnson, and to express their opinion as to the propriety of their continuance by order of Congress.

Resolved, That said committee consist of Dr. Morrel Wyman, of Boston; Richard C. Taylor, Esq., of Philadelphia; and Profs. B. Silliman, jun., and J. P. Norton, of New Haven.

Resolved, That the Committee do now adjourn, *sine die.*

APPENDIX.*

1. Corrected Report of Dr. Hare's Remarks at the Philadelphia Meeting of the Association in 1848.

Dr. Hare stated that he was desirous to correct an erroneous report which had been published in the proceedings of the Association, respecting some sentiments expressed by him at their meeting in Philadelphia, in September, 1848. This report had been copied from one published in the *North American;* although, soon after its appearance, a corrected statement of the language used had been published in the same channel.

Dr. Hare requested that the corrected statement might be permitted to appear in the forthcoming volume of the proceedings of the Association. Dr. Hare then read the corrected report which is subjoined :—

In the report of the proceedings of the American Association for the Advancement of Science, I am made to accuse our national administration in particular for not patronizing men of science. *This is altogether a misapprehension.* I made no allusion to any government in particular, but deplored, generally, the existence of a vicious propensity in mankind to expend blood and treasure in the acquisition of military glory, instead of aiming at the *higher* glory of successful zeal and ability in advancing science. I adverted to a remark made by Liebig, "that men of science had done immense good to the world, while the world had done very little for them :" that, in gratifying the passions, whims, and caprices of sovereigns and aristocrats, millions had been wasted or misapplied; while the scientific benefactors of the human race were, in most cases, obliged to struggle with penury, and to forego not only the enjoyments,

* Containing papers which were not received in time for insertion in their proper place.

but even the comforts of life, in order to command the means of prosecuting their investigations.

I adverted to the fact, that, although our political independence had been acknowledged more than half a century ago, our scientific and literary tutelage still endured. Having no tribunal at home of sufficient authority with the world of science, it was in Europe only that the stamp of comparative merit could be effectually applied to American authors. Crossing the Atlantic, the rays of American genius, if ever they reached the intellectual vision, even, of the more enlightened portion of the inhabitants of this continent, were indebted to the fortuitous assistance of a foreign mirror, reluctantly applied.

I advanced that want of the means of subsistence, "*poverty's unconquerable bar*," often deprived science of the services of those who would have been its brightest ornaments, by rendering an arduous devotion to a trade or profession indispensable to the attainment of a living.

Adverting to the beneficial influence of the West Point School in producing accomplished engineers, I urged that scientific talent and acquirement should, *cæteris paribus*, be a motive for military preferment; and that, during peace, the talented and sufficiently educated portion of our officers should be allowed, while supported by their pay, to employ their genius in scientific investigation.

From such a course, there would not only be the positive benefit arising from their scientific labors, but the negative benefit of the avoidance of the evils of idleness.

It might have been added that in this way they might have the glory of achieving an intellectual independence of the Old World, unaccompanied by the horrors of warfare.

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2. Report of Karl Culmann, Esq., of the Royal Bavarian Engineers, to Prof. A. D. Bache, LL. D., Superintendent of the United States Coast Survey, Aug. 15th, 1850, on the Coast Survey Experiments for Measurement of the Velocity of Propagation of the Hydro-galvanic Waves.

Cambridge, Mass., Aug. 15, 1850.

To Prof. A. D. Bache, LL. D., Superintendent U. S. Coast Survey.

Dear Sir:—In compliance with your invitation, I have the pleasure

of submitting the results of my examination of the registers which Mr. Walker has preserved of his experiments for measuring the wave-time or velocity of propagation of the hydro-galvanic current along the American telegraph lines. I have made careful measures of several groups of recording fillets, for the purpose of forming an independent judgment on the subject. The groups which I have measured are respectively as follows:—

1st. The registers of the Coast Survey work of October 31st, 1849. These were kept at Seaton Station, Washington, at Harper's Ferry, at Cumberland, Wheeling, and Cincinnati. The Wheeling register was too indistinct to admit of accurate measurement. The clock station was at Washington; the signal stations at Cumberland and Wheeling.

2d. Of the experiments of February 4th, 1850, at which I was present, by the invitation of Mr. Walker, I have measured two groups, one with the St. Louis and the other with the Louisville signals. The clock station was at Washington. The records were kept at Seaton Station, and at the Telegraph Office in Washington, and at Pittsburgh, Cincinnati, Louisville, and St. Louis.

My measures have been made both on the electrotome and electropœa scales. The pauses have been carefully compared together, in order to detect any accidental irregularity of the working of the instruments that would vitiate the result. The measures that have been used are those only which presented sufficient regularity in the duration of the printed clock and signal pauses. In conformity with the practice of Mr. Walker, I have taken the mean of the results by the electrotome and electropœa scale readings for a first discussion of the equations of condition. From a careful examination of the sources of irregularity in the pauses, it appears that the quantity u (viz., the inward armature vibration time), in Mr. Walker's paper, (No. 7 of the *Astronomical Journal*,) is more variable than y or z, and is the chief cause of the apparent irregularities in the readings of the electropœa scale. This circumstance has induced me, with Mr. Walker's approbation, to repeat the solution with only the electrotome readings. The general result is nearly the same in both processes, confirming Mr. Walker's opinion that these disturbing quantities, y, z, and u, are only accidental and evanescent in the mean of a greater variety of experiments. In consequence of the greater uniformity of the electrotome readings, these results appear to be more convergent towards the truth than the average of the two kinds of scales. Accordingly, I subjoin the particulars of the electrotome readings, and the results of their discussion. The analytical formulæ which

I have used are the same as those of Mr. Walker in the *Journal,* No. 7. Thus, in his nomenclature,

$$b - b' = (a_{\mathrm{I}} - a) - (a_{\mathrm{I}}' - a')$$

There are always two directions in which the wave from the signal station departs towards the receiving station. The two spaces traversed make the entire circuit. The one is the complement of the other. Mr. Walker, in forming his conditional equations, has always taken for a the value corresponding to the shortest junction line. This is the only admissible hypothesis where the line is homogeneous. But where, as in the Coast Survey experiments, two-fifths of the circuit are through the ground, and the remaining three-fifths by the iron wire, if the velocity is slower in the ground than in the iron wires, it may happen that the receiving magnet first obeys the impulse from the wave coming through the longer portion of the circuit. To avoid prejudging this question, let us call

x = wave time per mile in the iron wire.
x' = " " " ground circuit.
g' = the miles of the ground circuit.
$t = g'\,x'$ = wave time through the ground.
$g = \frac{t}{x} = \frac{x'}{x}\,g'$ = the number of miles of iron wire that have the wave time t.

Then the performance of the telegraph circuit in propagating the galvanic waves will be the same as that of a homogeneous iron wire line, having g miles as a substitute for the ground portion of the circuit. Then we have in the first experiment Oct. 31 1849, where the wire line connected Seaton Station, Harper's Ferry, Cumberland, Wheeling and Cincinnati, for the whole length of an equivalent homogeneous iron wire circuit $578 + g$, and we have for the distance a from each station to the clock station, two complementary values as follows,

Stations.		a
(Seaton Station,)	*S.*	0 0, or $578 + g$
(Harper's Ferry,)	*H.*	74, or $504 + g$
(Cumberland,)	*Cd.*	165, or $413 + g$
(Cincinnati,)	*C.*	578, or g

In this homogeneous circuit, (the wave always reaching the receiving magnet through the shortest junction line), it is clear by the inspection of the above values of a, that the wave proceeding from the clock

station will always take the wire line for *H* and *Cd*; but for *C* it will take the ground line if g is less than 578. Also for the route of the inducing wave from the signal station, *Cd.* to each other station.

Stations.			a
S.	-	-	165, or $413 + g$
H.	-	-	91, or $487 + g$
Cd.	-	-	0, or $578 + g$
C.	-	-	413, or $165 + g$

By inspection of this table it appears that the wave from the signal station (*Cd.* = Cumberland) passes through the wire to *H.* and *S.*; but to *C,*

through the wire if $413 < 165 + g$; or $g > 248$
ground if $413 > 165 + g$; or $g < 248$

Accordingly we may make two hypotheses.

Hyp. I. That g falls between 248 and 578
II. below 248.

whence for $(a_1 - a)$

Stations.	a_1	a	$a_1 - a$
S.	165	0	165
H.	91	74	17
Cd.	0	165	—165
C.	413	g	$413 - g$

and we get the value of $\bar{a}$ and n in Mr Walker's conditional equation (XIV), where,

$$0 = \bar{a}\, x + n$$
$$\bar{a} = [(a_1 - a) - (a_1' - a')]$$

by taking $\bar{a}$ from the above tables and that of $n = (b^1 - b_1)$ from the list of measures, in the electrotome scale, and putting t for $g\ x$, we have,

			No. of Comparisons.
1	*S.* — *H*;	$o = + 148\ x \qquad - 0{\cdot}00239$	23
2	*S.* — *Cd*;	$o = + 330\ x \qquad - 0{\cdot}00657$	34
3	*S.* — *C*;	$o = - 248\ x + t + 0.00671$	29
4	*H.* — *Cd*;	$o = + 182\ x \qquad - 0{\cdot}00173$	22
5	*H.* — *C*;	$o = - 396\ x + t + 0{\cdot}00236$	25
6	*Cd.* — *C*;	$o = - 578\ x + t + 0{\cdot}00002$	36
		Total,	169

whence by

$$
\begin{array}{ll}
1, & x = +\ 0{\cdot}0000702 \\
2, & x = +\ 0{\cdot}0000585 \\
4, & x = +\ 0{\cdot}0000926 \\
3 \text{ and } 5, & x = +\ 0{\cdot}0000434;\ t = -\ 0{\cdot}0461 \\
3 \text{ and } 6, & x = +\ 0{\cdot}0000522;\ t = +\ 0{\cdot}0301 \\
5 \text{ and } 6, & x = +\ 0{\cdot}0000700;\ t = +\ 0{\cdot}0404
\end{array}
$$

whence, mean result, $x = +\ 0{\cdot}0{\cdot}0006450$

$$\frac{t}{x} = g = \quad 603 \text{ miles}$$

$$\text{wave space in wire } m = \frac{1}{x} = 15504 \text{ miles per second}$$

$$\text{" \quad " ground } m' = \frac{403}{t} = 10{\cdot}360 \quad \text{"}$$

403 miles being the ground line from Washington to Cincinnati. From this result it appears that the velocity through the ground is only two thirds of the velocity through the iron wires; so that both hypotheses I. and II. fail, and the inducing wave between Washington and Cincinnati appears to have traversed the wires 587 miles, rather than through 603 miles, the equivalent for the ground.

Let us now form the conditional equations for this last interpretation of the problem, and we have

Stations.	a	a_1	$a_1 - a$
S.	0	165	+ 165
H.	74	91	+ 17
Cd.	165	0	— 165
C.	578	413	— 165

preparing the six equations as before with the new values of a_1 those of n remaining as before, there results,

$$x = +\ 0{\cdot}00005874 = \text{wave time in wires per mile.}$$

$$\frac{1}{x} = m = \quad 17024 \text{ miles} = \text{wave space per second.}$$

This is the most plausible interpretation of the experiment of Oct. 31, 1849, with the Cumberland dots. If the method of Mr. Walker had been adopted, that of using the mean of the electrotome and electropœa scales, we should have concluded that

$$x = + 0000439.$$

$$\frac{1}{x} = m = 22766 \text{ miles in wires.}$$

$$\frac{403}{t} = \frac{1}{x^1} = m' = 35526 \text{ miles in ground.}$$

If we treat the experiment with the Wheeling signals, on the 31st of October, in the same way, we find by hypothesis I. the conditional equations from comparisons of the electrotome readings—

				Comparisons.
1	S—H	$0 = + 148 \times x$	$+ 0^s{\cdot}0122$	9
2	S—Cd	$0 = + 330 \times x$	$- 0{\cdot}0167$	17
3	S—C	$0 = + 40 \times x + t$	$- 0{\cdot}0251$	12
4	H—Cd	$0 = + 182 \times x$	$- 0{\cdot}0382$	5
5	H—C	$0 = - 108 \times x + t$	$- 0{\cdot}0431$	9
6	Cd—C	$0 = - 290 \times x + t$	$- 0{\cdot}0038$	12
			Total,	64

$$t = + 0{\cdot}0396.$$

$$x = + 0{\cdot}0001043.$$

$$x = + 0.000983.$$

$$\frac{1}{x} = m = \quad 9585 \text{ miles in wire.}$$

$$\frac{403}{t} = \frac{1}{x'} = m' = \quad 10176 \text{ miles in ground.}$$

The absence of the Wheeling readings (the signal station,) diminishes the weight of these results.

I come now to the experiment of Feb. 4th, 1850, made by Mr. Walker, on the line between Washington and St. Louis. I have carefully measured the electrotome and electropœa readings for one minute of the St. Louis, and one minute of the Louisville Signals, both on Seaton clock scale as recorded at Seaton Station, Washington Office, Pittsburgh, Cincinnati, Louisville, and St. Louis. The pauses are more uniform than on the 31st of October, 1849, and the successive measures far more concordant.

I have treated the conditional equations for the experiments by the two hypotheses, supposing the St. Louis magnet to be worked respectively by the ground currents, and by that of the wire. In the case of the St. Louis Signal, the velocity in the wires (10,600 miles per second,) is so small, that the ground current appears to have arrived first at St.

Louis. In the case of the Louisville Signals, the velocity in the wires (13,700 miles per second,) is so much greater, that all the magnets appear to have been worked by the wire current.

The following tables contain the results by the hypotheses. The distances of the several stations from Seaton Station, are respectively—

Seaton Station,	S.	0	miles.
Washington Office,	W.	1	"
Pittsburgh Office,	P.	288	"
Cincinnati Office,	C.	622	"
Louisville Office,	Lv.	747	"
St. Louis Office,	S. L.	1045	"

The distance (W—S) = one mile has been neglected, and the mean of the results for both stations taken as a single record. With the other distances the coefficient [$(a_1—a)—(a_1'—a')$], in the manner already described.

St. Louis Signals on Seaton Clock Scale.

No.	Stations Composed.		Conditional Equation.		No. of Comparisons.
1	S—P	;	$0 = t - 469\,x - 0{\cdot}0618$	;	29
2	S—C	;	$0 = t + 199\,x - 0{\cdot}0908$	;	37
3	S—Lv	;	$0 = t + 449\,x - 0{\cdot}1373$	;	42
4	S—S.L.	;	$0 = 2t \quad - 0{\cdot}1451$	;	37
5	P—C	;	$0 = \quad 668\,x - 0{\cdot}0364$	;	23
6	P—Lv	;	$0 = \quad 918\,x - 0{\cdot}0851$	;	23
7	P—S.L.	;	$0 = t \quad 469\,x - 0{\cdot}0963$	;	21
8	C—Lv	;	$0 = \quad 250\,x - 0\;0368$	;	35
9	C—S.L.	;	$0 = t \quad 199\,x - 0{\cdot}0502$	;	28
10	Lv—S.L.	;	$0 = t \quad 449\,x - 0{\cdot}0148$	;	22

Whence there results, mean value of $x = 0{\cdot}00009388$.

$$t = 0{\cdot}07455.$$

$$\frac{t}{x} = g = 794.$$

Velocity of the current in the wires, $\frac{1}{x} = 10652$ miles per second.

" " ground, $\frac{741}{t} = 9945$ " "

Louisville Signals on Seaton Clock Scale.

No.	Stations Composed.		Conditional Equation.		No. of Comparisons.
1	S—P	;	$0 = 576\,x - 0{\cdot}0073$	;	18
2	S—C	;	$0 = 1244\,x - 0{\cdot}0723$	;	19
3	S—Lv	;	$0 = 1494\,x - 0.1048$	;	17
4	S—S.L.	;	$0 = 1494\,x - 0{\cdot}0954$	:	17
5	P—C	;	$0 = 668\,x - 0{\cdot}0531$	;	14
6	P—Lv	;	$0 = 918\,x - 0{\cdot}0874$	;	15
7	P—S.L.	;	$0 = 918\,x - 0{\cdot}0787$	;	14
8	C—Lv	;	$0 = 250\,x - 0{\cdot}0286$	;	16
9	C—S.L.	;	$0 = 250\,x - 0{\cdot}0246$	;	16
10	Lv—S.L.	;	$0 = 0 - 0{\cdot}0018$	;	16

Whence there results—mean value of $x = 0{\cdot}00007092$.

Velocity of the current in the wires, $\frac{1}{x} = 14100$ miles per second.

Combining together all the different values of wave space per second, we find the following results:—

Clock Station.	Signal Station.	Velocity in miles per second. In iron wire.	In the ground.
Seaton Station,	Cumberland,	17024	——
"	Wheeling,	10176	9535
"	Louisville,	14100	——
"	St. Louis,	10652	9940
	Mean values	13000	9740

These are the mean results of all the electrotome readings, which I measured, during the time that I could devote to the subject. I have repeated the measures for the electropœa readings, and from the mean of the readings on both scales, find very nearly the same result as the above, which are for the electrotomes. But as the electrotome readings differ much less from each other than those of the electropœa, they are always to be preferred, particularly when the number of comparisons is not very great. The latter have, with Mr. Walker's consent, been quite omitted in the present discussion.

A few days since, Mr. Walker had the kindness to communicate to me all the readings made by himself and his assistants, in the course of the experiments made by the Coast Survey, up to this time, for the measurement of the velocity of the galvanic current.

His general result did not differ much from that of my own measures and computatious for a part of them. He makes the velocity in the iron wires a little greater than mine. His computations give no absolute value for the velocity of the galvanic waves in the ground; but only the upper limit, which agrees with mine. A smaller velocity of the waves in the ground, if such be really the case of nature, would exclude them from all influence on the magnets on the Coast Survey, experiments yet made. To determine the lower limit of the velocity in the ground, requires a greater contrast of spaces, respectively, in the iron wires and ground. Mr. Walker's upper limit is 11,200 miles per second for the velocity in the ground. The two results are, therefore, not at all contradictory to each other. Indeed, it is not impossible, that the point at which the two currents starting from one and the same point in the circuit, meet each other in the opposite part of the circuit, may vary within a wire or ground space (per second), of several hundred miles. This seems to be the natural inference from all the experiments yet made. Suppose, now, that one of the recording stations falls between these limits, and we may perceive at once, that the same magnet may be worked sometimes by the ground wave and sometimes by the wave through the iron wire.

Mr. Walker's mean value for galvanic wave space per second in iron wires is 15,400 miles. Mine is 13,000, differing about one sixth part from his. it would seem, therefore, that the meeting point of the two currents, which start from the same point at the same time, may vary within the space of $\frac{1045}{6}$ or 170 miles.

The fact of the crossing of the waves on the wire from Washington to St. Louis, *in transitu*, first noticed by Mr. Walker, is perfectly well established in many cases, and in many others it would be more evident, if the armature motions for the different crossing dots had not so much influence upon each other.

Yours, respectfully,

KARL CULMANN.

3. NEW TEST FOR THE NITRITES AND NITRATES. By GEORGE C. SCHAEFFER, Prof. of Chem. and Nat. Phil., Centre Coll., Danville, Ky.

Chemistry has hitherto furnished no distinctive test for the nitrites, when present in small quantities. From the supposed unfrequent occurrence of these salts, the want of such a test has never been felt.

For several years I have been engaged in a research which has led me to believe that the nitrites are of far more frequent occurrence than is commonly supposed, and that they have been mistaken for nitrates, as the usual process, with pure sulphuric acid and protosulphate of iron, will give the same reaction with both classes of salts. In order to settle the question, it was necessary to find a distinctive test, which should avoid all risk of confusion. I first tried acetic acid, which, as is well known, does not act upon the nitrates, while it decomposes nitrites. By substituting this acid for the sulphuric in the usual process, I succeeded perfectly. The test seemed even more delicate than that for the nitrates, and was more elegant in its operation.

Still the following difficulties were to be encountered. The slightest quantity of peroxide in the sulphate gave a color with acetic acid, and injured the delicacy of the test. Moreover, the nitrites are generally either destroyed or converted into nitrates, with such readiness, that it would be almost impossible to concentrate their solutions.

At last I was led to the following process, which leaves nothing to be desired. To the solution supposed to contain a nitrite, add one or two drops of solution of yellow prussiate of potash—there should not be enough to give a perceptible tinge to the liquid. A few drops of acetic acid are then to be added, and immediately, or in a few minutes, according to the quantity of nitrite present, the liquid assumes a rich yellow tint.

As the re-agents used give nearly the same color, spontaneously, after some time, even in pure water, it is better, when testing for minute quantities, to use two similar vessels, one containing pure water, and the other the liquid under examination, to both of which the re-agents are to be added in precisely equal quantities. The vessels should be equally exposed to the light, with a sheet of white paper behind them.

With these precautions I have found this test astonishingly delicate, in fact, ranking with those for iron, iodine, &c. Using fused nitre, I have detected the presence of 1 pt. in 617,000 pts. of water; a bystander wholly ignorant of the nature of the operation, pronouncing as

to the color. Yet this salt contained about one half its weight of undecomposed nitrate.

It should be remarked that the presence of a large quantity of nitre has no influence upon this test, as with pure water it gives no color. The same reaction answers also for the hyponitrates.

The next step is to convert this test into one for the nitrates. The decomposition of the nitrates of lead and mercury by boiling with excess of their respective metals, has long been known. The reaction of metals with the alkaline nitrates does not seem to have been studied. I find that nitrate of ammonia is readily decomposed in presence of metallic lead, and what seems surprising, nitrate of potash is also decomcomposed, though not so readily.

To test for the nitrates, we have only to agitate the slightly warmed liquid, for a few minutes, with shavings of lead, and proceed as before. By a longer digestion more of the salt would be converted into nitrite, and the color would be stronger. In estimating the delicacy of this process, I had used pure rain water, but before completing the experiments I was obliged to be absent for several days; on my return, I soon found that the water from the same cistern contained so much of nitrates and nitrites, that it could no longer be used. From the readiness with which the decomposition was effected, I presume that nitrate of ammonia was present. The interval had been marked by the occurrence of frequent and severe thunder showers.

By using distilled water, I have been able to detect the presence of 1 pint of nitre in about 60,000 of water, digesting with lead for only a short time. Mercury containing a minute quantity of lead, seems to answer better than lead, for a time, in the above process; but after long use it loses its effect. It will not answer to add to it any perceptible quantity of lead, as the liquid then becomes muddy on agitation.

Oxalic, tartaric, and dilute hydrochloric acids may be substituted for the acetic, except when they produce precipitates, which would destroy the clearness of the liquid.

The yellow color produced in this test is the result of a complex decomposition. Everitt's yellow salt, and red prussiate, seem to be formed, and in some cases very minute quantities of Clayfair's nitro-prusside.

4. ON THE STRUCTURE AND AFFINITIES OF THE GENUS BATIS OF LINNÆUS. By Dr. J. TORREY.

The *Batis maritima* (which is the only species of the genus) is a common shrubby plant of the West India Islands, and the neighboring parts of the continent; growing in sand along the sea-shore. It is surprising that no correct description of its flowers and fruit has hitherto been published. Lindley says, "that British botanists should be ignorant of the structure of one of the commonest plants of one of their oldest colonies, is certainly a thing not to be proud of."

It appears to have been first noticed more than one hundred and fifty years ago, by Sloane, in his catalogue of the plants of Jamaica, under the name of "*Kali fructicosum coniferum, flore albo.*"

P. Browne, in his Civil and Nat. History of Jamaica (1756), first gave this plant the name of Batis; and his description was very good for that time.

Linnæus, in 1763, briefly characterized the plant from Browne, but gave no additional information respecting it.

Jacquin shortly afterwards gave a detailed description which was correct, except as relates to the flowers and seeds.

Jussieu has drawn his character of Batis from Sloane and Browne, and seems to have been unacquainted with the plant, except from the description of those authors. He has left it among his "*genera incertæ sedis.*"

Indeed no botanical writer, since Jacquin, has made any positive addition to our knowledge of this common shrub, except Lindley. In his remarks on the genus *Sarcobatus* of Nees, (*Lond. Jour. Bot.*, 1845,) he correctly describes the structure of the ovary, and rendered it probable (for his specimens were not mature) that what had been described as seeds by former botanists, were only the empty and easily separable cells of the fruit; for the plant seems rarely to perfect its seeds.

Several years ago the Batis was detected at Tampa Bay, East Florida, by Dr. Leavenworth, and shortly afterwards at Key West, by Mr. Blodgett. From the latter gentleman I have received the ripe and perfect fruit, preserved in spirits. With these ample materials, I am able to give a more complete description of this interesting plant than has yet been published, and to determine, with considerable certainty, its affinities. I can only notice in this abstract the more important points of structure that were overlooked by previous observers. The stami-

nate flowers, instead of being apetalous, as was formerly supposed, are furnished with a distinct corolla of four petals. In the fertile spike, each flower consists of a pistil subtended by a deciduous bract; the calyx and corolla not being evident. The ovary is uniformly four-celled, with a single erect anatropous ovule in each cell. There is no style, and the rather large sessile stigma is somewhat two-lobed. The fruit consists of from eight to twelve pericarps united into an oblong tuberous compound drupe. The separate pericarps are four-celled, with a tough, hard endocarp. Each cell contains an oblong seed, the embryo of which is destitute of albumen.

There has been much speculation as to the affinities of Batis. Some of the most eminent modern botanists have retained it among unclassed plants. Others have placed it in the Goose-foot tribe. Sprengel doubtfullyreferred it to the Pine tribe. Martius between the Willows and Podostemums. Meissner stationed it immediately after the Nettles. To none of these families does it seem to be allied. Lindley, in his latest work, "The Vegetable Kingdom," referred it provisionally in the Euphorbial alliance, and conjectured, with much sagacity, that when better known it might be placed in the Empetrum family. With this it agrees in its dioecious flowers, definite stamens, several-celled ovary, solitary erect anatropous ovules, and inferior drupe-like fruit. It differs, however, in habit, in the want of imbricated scaly sepals or bracts, in the presenceof a true corolla, and especially in the want of albumen. So that it may be placed as a small distinct order or sub-order between Euphorbiaceæ and Empetraceæ.

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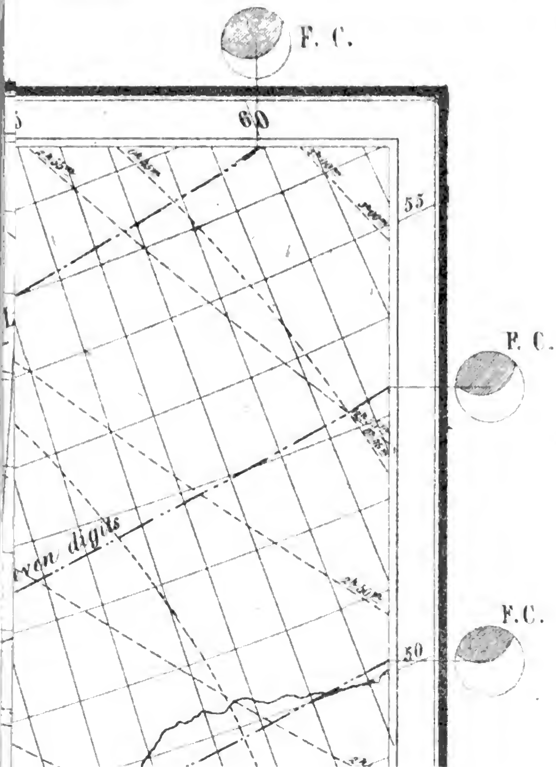
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